Urban transport externalities

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To my family, to my brother and to my beloved Natalia

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1 Introduction

Cities are at the heart of all economic activity, serving as the backdrop against which people, firms and governments interchange commodities, services, labour, technology and ideas. Most of these transactions require the transportation of either goods or people. Yet, while transportation is essential for providing a city's essential functions, the users of transport infrastructure (above all, road users) generate externalities that are imposed on other road (and non-road) users. Traffic congestion, accidents and environmental pollution have been identified as the three most important negative externalities associated to car travel (Shefer and Rietveld, 1997). Indeed, these externalities have become critical issues at a moment in which the global increase in urban population is creating a rising demand for urban transportation and when environmental problems (e.g. air pollution and the preservation of open spaces), in addition to other major urban costs (e.g. traffic congestion and the lack of affordable housing), have to be tackled. In parallel with these developments, however, there are other externalities — produced by more sustainable modes of travel, for example walking, and associated with shopping — that are positive and which are especially salient in Europe's city centres.

In Europe — which constitutes the main focus of this PhD dissertation — mitigating urban costs and promoting vibrant and sustainable cities are at the top of the EU policy agenda. The following quotation, an extract from a Communication from the European Commission to the Council and the European Parliament on urban transportation, stresses the EU's commitment to these issues.

> *"80 percent of Europeans live in an urban environment. Public transport, cars, lorries, cyclists and pedestrians all share the same infrastructure. Urban transport accounts for 40 percent of CO_2 emissions of road transport and up to 70 percent of other pollutants from transport. One in three road fatalities occurs in cities. Congestion problems, too, are concentrated in and around cities. How to increase mobility while at the same time reducing congestion, accidents and pollution is the common challenge to all major cities. More than anyone else, city dwellers directly experience the negative effects of their own mobility and may be open to innovative solutions for creating sustainable mobility."* (Commission of the European Communities, 2006)

As Combes et al. (2016) report, the literature on agglomeration economies is now well established (see Rosenthal and Strange (2003); Puga (2010); Combes et al. (2011); Combes and Gobillon (2015), for reviews); yet, little is known about urban costs. In this regard, cities incur both pecuniary costs — such as, high housing prices and long commutes, and non-pecuniary costs — such as, pollution and crime (Duranton, 2014). This PhD dissertation focuses on the estimation of transport-related urban externalities and the interaction between different externalities. Estimating transport-related non-pecuniary externalities has been recognised to be crucial for maximising welfare (Pigou, 2013), while the interaction between different externalities is important for two main reasons. First, if two externalities are causally related (e.g. accidents and traffic congestion), then a policy aimed at reducing one of them can have multiplicative benefits (referred to as "co-benefits" by Proost and Van Dender (2012)) for society. Second, overlooking the interaction between different externalities can have unexpected outcomes when policies are implemented. For example, Bento et al. (2014) demonstrate the critical importance of the interaction between the introduction of the Clean Air Vehicle Stickers policy in California and unpriced congestion, showing that the policy generates substantial welfare losses at the expense of the policy's expected primary welfare gain.

Chapters 2 and 3 of this dissertation study the externalities of cities located across the length and breadth of the European continent. Chapter 2 focuses on the impact of highway and railway development on the suburbanization of European cities, while Chapter 3 analyses the effects of highway construction on urban congestion and, subsequently, on air pollution. While these externalities have been analysed to some degree in the US, to the best of my knowledge, no study to date has attempted to analyse the European system of cities as a whole.

Transportation, and highways in particular, are as salient a phenomena in Europe as they are in the US. The transport sector as a whole typically represents around five percent of gross domestic product (GDP) in both the US and Europe, and transport networks, primarily highways, account for some of the largest investments ever made (Redding and Turner, 2015). The average annual cost of road investments in the EU28 over the period 1996-2014 was approximately €58 billion in 2015 prices, that is, about 0.3 percent of GDP in 2015 (compared to €61 billion equivalent or 0.4 percent of the GDP in the US)[1] (OECD, 2017). The highway network in Europe grew immensely during the second half of the 20th century, from 259 km in 1955 to 67,779 km in 2011, with much of this development being financed by the EU Re-

[1] Note, however, that in Europe, 64 percent of the total highway network in 2010 was constructed in the period 1955-1990.

gional and Cohesion Funds[2]. At the same time, EU policies have sought to mitigate the problems that the literature has identified as potential externalities of highway construction, namely, suburbanization (Baum-Snow, 2007), traffic congestion (Duranton and Turner, 2011), air pollution, CO_2 emissions, energy inefficiency (Glaeser and Kahn, 2010) and social segregation[3] (Glaeser and Kahn, 2004).

Although Europe and the US have many features in common, European cities present a series of unique characteristics that make them particularly interesting to study. First, cities in Europe are more compact. According to the OECD (2011), the average urban population density of the European metropolitan areas was 718 persons per km^2, compared to just 282 in the US. Second, car use in Europe is relatively low (about 42 percent lower than in the US) (Eurostat and OECD, 2011), while public transportation flows, in particular rail passenger transport, are much higher in the EU than in the US (391.8 vs. 10.3 billion passenger-km in 2015) (International Union of Railways, 2015). Europe is also the world's leader in rapid transit systems. According to Gonzalez-Navarro and Turner (2016), the number of subway km per capita in European cities is more than twice that of their North American counterparts (1.9 compared to 0.9 km per 1,000 inhabitants). Third, in Europe, unlike in the US, upper- and middle-class households live in the city centres [4] (Glaeser et al., 2008). This difference in the dominant urban spatial structure can be explained theoretically by the distinct endowments of both historical and other urban amenities in Europe's city centres (Brueckner et al., 1999). Indeed, their historical amenities are particularly predominant, while land-use regulations, especially in Western Europe, protect open-space and historic districts.

Chapter 2 of this dissertation estimates the joint causal effect of highway and railway infrastructure on the suburbanization of population in European cities. The countries and regions of Europe followed quite distinct paths of development and urbanization during the late twentieth century. For example, the suburbanization of Northern and Southern European cities were very different processes, while the planned Eastern European countries were suddenly exposed to market forces that

[2]During the first 15 years of its existence, the European Regional Development Fund devoted 80 percent of its funding to infrastructure projects (Vickerman, 1991) and in the period 2000-2006 about 35 percent of the Structural Funds and 50 percent of the Cohesion Fund were spent on infrastructure projects (Crescenzi and Rodríguez-Pose, 2012). During the period 2007-2013, again, approximately 35 percent of the total amount spent by the Structural and Cohesion Funds was invested in roads, mainly highways (DG-REGIO, 2016).

[3]The *Europe 2020* strategy focuses on reducing CO_2 emissions and increasing energy efficiency; fighting social exclusion; and promoting education and R&D. Although the last two areas might seem irrelevant to the discussion here, they reflect typical criticisms levelled at the allocation of EU funding, often believed to favour 'hard' (e.g. highways) as opposed to 'soft' infrastructure (e.g. human capital) investments.

[4]This trend is also related to the discussion in the recent literature on urban renewal in the US (Couture and Handbury, 2015; Baum-Snow and Hartley, 2016; Diamond and McQuade, 2016)

'demanded suburbanization' at a relatively late date (Leontidou, 1990). Moreover, the expansion of the highway network cannot be considered in isolation in Europe, given the prominent role played by the continent's railways since the nineteenth century. Indeed, the share of railroads has recently increased considerably, reflecting EU objectives for a Single European Railway Area (European Commission, 2010).

While many detailed studies of small areas or of a single country have been reported, few examine broad cross-sections of cities and none, to the best of my knowledge, analyses cities across various countries. One of the main problems impeding such analyses at the European level is the lack of harmonized urban data. In Chapter 2, I am able to overcome this problem by employing GIS software and techniques to extract information from maps, some of which date back to previous centuries. A further challenge arises from the endogeneity embedded in estimates of the effect of transport infrastructure on suburbanization. I address this by means of instrumental variables regressions, using the post routes in 1810 and the railroads in 1870 as instruments for highways and railways, respectively.

Using a unique dataset of 579 European cities from 29 European countries during the period 1961-2011, I provide evidence that an additional highway ray displaces on average approximately 9 percent of the central city population to the suburbs in Europe's cities. In contrast, when considering both highways and railways jointly, I find no significant results for the effects of railways on suburbanization. This result highlights the significance of jointly considering the effect of both types of transport infrastructure and represents an important contribution of this research. Moreover, the effect of highways on suburbanization exhibits considerable heterogeneity. Highways caused more suburbanization in the period 1961-1981, when urban growth in Europe was at its peak. However, Roman and Medieval cities appear to be more resilient to this process. Indeed, this existence of historical amenities in the cities of Europe appears to provide a reasonable explanation for these differences, providing some of the first empirical evidence for Brueckner et al. (1999)'s theory.

While suburbanization is beyond doubt an important externality associated with the motor vehicle, air pollution is arguably the most prominent because of its well-documented adverse effects on human health. Air pollution kills 3.3 million people, mostly in cities, every year according to figures reported in Lelieveld et al. (2015), while the International Energy Agency (2016) reports around 6.5 million premature deaths attributable to air pollution. In 2005, the European Commission responded to this threat by introducing its Clean Air Directives, which directly apply to Europe's cities. These regulations mean when cities violate the maximum allowable limits, mayors and local governments are required to develop clean air action plans (APs) if they want to avoid huge financial sanctions[5] (Council Directive 2008/50/EC, 2008).

Traffic congestion in Europe, concentrated above all in the continent's cities (Christidis and Ibáñez Rivas, 2012) is another major issue, with costs estimated at over €110 billion a year (about 1 percent of GDP). According to INRIX and Cebr (2014), the cost of traffic congestion in France, Germany, the UK and the US between 2013 and 2030 is expected to rise by 50 percent. Based on these forecasts, the total cumulative cost of traffic congestion for these economies during these years is estimated to be about $4.4 trillion, without taking into account the cost of air pollution and CO_2 emissions. As such, analysing the effect of vast investments in highway infrastructure on traffic congestion, as well as on air pollution, is clearly of great importance.

The effect of increasing the supply of highways on the level of traffic congestion, that is, the 'fundamental law of highway congestion' — namely, that the speed on an expanded highway will revert to its previous level before the capacity expansion (Downs, 1962, 1992), has already been tested empirically in the context of the US (Duranton and Turner, 2011) and Japan (Hsu and Zhang, 2014). However, it is not immediately clear that these results should be directly transferable to Europe. As mentioned, car use in Europe is markedly lower than in the US and public transportation and alternative modes of travel are popular on the old continent. Therefore, the applicability of the 'fundamental law' in Europe's cities has remained an open question until now. Confirmation of the 'fundamental law' would mean that the vast amounts of EU resources allocated to highway construction in recent decades have been ineffectual in reducing traffic congestion. Moreover, we would also expect to find an indirect effect of highway investments on air pollution, as a result of the increase in traffic following the building of more highways. While there is a growing literature that analyses the impact of government regulations on air pollution and human health (Chay and Greenstone, 2003, 2005; Currie and Neidell, 2005; WHO, 2016), the findings from the literature analysing the impact of transportation on air pollution, especially at the urban level, remain inconclusive. Small and Kazimi (1995) report heterogeneous estimates of the cost of air pollution over time and in association with different vehicle categories. Gallego et al. (2013) and Bel and Rosell (2013) find that certain policies aimed at reducing car use might have adverse effects on air pollution, whereas Hilber and Palmer (2014) find that car use decreases air pollution in a global sample of cities. Finally, there is a small strand of literature that studies the effect of subways and highway tolls on air pollution (Gendron-Carrier et al. (2016) and Fu and Gu (2017), respectively).

Chapter 3 of this dissertation tests and confirms the 'fundamental law of highway

[5] e.g. Leipzig had to pay €700,000 per day (Wolff, 2014) for repeatedly violating the 35-day limit rule.

congestion' for the cities of Europe. The identification strategy used in this chapter is based on panel data techniques and four different historical transportation networks in Europe. The latter are combined to construct a valid instrument that can explain the highway network over the whole European continent. Using different approaches, I find an elasticity of Vehicle Kilometres Travelled (VKT) with respect to highway lane km in the range of 0.7-1. This elasticity suggests that the expansion of the highway network caused a proportional increase in traffic; thus, the average level of traffic congestion remained roughly unchanged. In a second stage, I estimate the effect of the increase in highway traffic on the emissions of some of the most harmful air pollutants. For nitrogen oxides, the estimated elasticity is approximately 0.10 — i.e. a ten-percent increase in highway traffic causes a one-percent increase in nitrogen oxide emissions. Sulphur dioxide also seems to increase considerably with highway traffic. Furthermore, the heterogeneous analysis shows that the increase in traffic congestion and urban air pollution is higher in cities without tolls — a finding that substantiates congestion pricing — and in cities without subways — a finding that corroborates rapid transit policies.

Finally, I derived a back-of-the-envelope calculation in an attempt at endowing the results on air pollution with an order of magnitude. In line with this calculation, the cost of air pollution attributable to the new highways built in Europe's cities in the period 1981-2001 was €6.3 million, which is arguably quite small. To put this number in context, I provide some background information. Based on the emission data I use, air pollution attributed to road transport fell by almost 50 percent in the cities of Europe during the period under study. This huge reduction in emissions was mainly driven by the EU Air Quality Standards, which by 1992 had already set threshold limits on several emissions. Indeed, the greatest effects of technology changes and end-of-pipe (EOP) control measures were observed in the road sector in the EU in the period 1970-2010 (Crippa et al., 2016). Thus, it is my contention that the cost of increasing the supply of highways has been relatively small (only 2.43 percent) compared to the benefits of actual improvements in fuel technology and the regulations introduced in the same period.

As stated at the beginning of this Introduction, traffic congestion, accidents and environmental pollution are the three main negative externalities related to car travel (Shefer and Rietveld, 1997). In this regard, Chapter 3 focuses on the effect of highway construction on traffic congestion, as well as the indirect relationship between highway congestion and air pollution, given the strength of the interaction effect recognised between these externalities (Proost and Van Dender, 2012; Bento et al., 2014). Chapter 4, in contrast, analyses the bidirectional relationship between highway accidents and traffic congestion for highways in England. Here, in order to capture the scale of these effects accurately, I am required to adopt a decidedly micro approach: the impact of an accident on traffic congestion is an impact that is only relevant for a relatively short time after the accident, in a relatively small area centred on the site of that accident. Consequently, I set up my research design using standard dynamic panel techniques adapted in such a way that they can exploit spatial 'big data'.

Given that open-source data are becoming increasingly available at the city level and that 'smart cities' are called on to make fast, real-world decisions about transport issues, the use of big data in the economic analysis of transportation is a field with great potential.

While many scholars have studied the effect of traffic congestion on road accidents since the '70s (Vickrey, 1968, 1969; Dickerson et al., 2000; Noland and Quddus, 2005; Quddus et al., 2010), only limited attention has been paid to the inverse relationship. The main hurdle impeding such analyses has been data availability and the inherent endogeneity of the relationship: road accidents typically occur in periods of high congestion; while accidents result in traffic congestion. Moreover, both congestion and accidents are affected by several observable and unobservable factors (e.g. weather, road conditions, speed limits, construction works, holidays, major events). These factors could give rise to concerns about endogeneity, suggesting that the identification of a causal relationship between road congestion and road accidents is a non-trivial issue.

The existing literature on the effect of accidents on traffic congestion (Vitaliano and Held, 1991; Skabardonis et al., 2008; Adler et al., 2013) has identified some of these endogeneity concerns, although they have not always been addressed adequately. This chapter estimates the effect of an accident on average flows, speeds and journey times, drawing on the observed patterns of traffic flows on England's highways in the period 2012-2014. Employing a panel data methodology that has previously been used to analyse electricity day-ahead market prices (Huisman et al., 2007) and the work of Adler et al. (2013), I take advantage of the stable periodic patterns of road traffic and the richness of information in the big data to estimate the causal effect of accidents on traffic congestion and vice versa.

A positive relationship between highway accidents and traffic congestion would mean that policies aimed at reducing one of them could have multiplicative benefits in terms of welfare. To identify both effects of this two-way relationship, I use dynamic panel data techniques and open access 'big data' of highway traffic and accidents in England for the period 2012-2014. The research design is based on the daily and hourly specific mean reversion pattern of highway traffic, which can be used to define a recurrent congestion benchmark. Using this benchmark, I am able to identify the causal effect of accidents on non-recurrent traffic congestion. The results of this analysis suggest that a marginal decrease in the average speed due to an accident is about 7.8 km/h, while the journey time increases by around 27 percent when I consider the duration of this effect. Another important finding is that the effect declines by 70-75 percent after the first quarter of an hour.

Additionally, I explore the 'rubbernecking' effect[6], as well as heterogeneous effects on the most congested highway segments. I then test the use of methods that employ the bulk of information in big data and methods that use relatively 'small data'. Both approaches produce very similar results. Finally, I find no evidence of a positive effect of traffic congestion on the probability of an accident. On the contrary, I find evidence of a non-linear convex negative effect, i.e. more congestion is associated with a reduction in the probability of an accident. These results suggest that policies that aim to reduce the probability and the number of accidents can be expected to have multiplicative benefits, while policies that seek a reduction in congestion are not expected to reduce accidents considerably. Finally, a back-of-the-envelope calculation suggests that an accident causes on average a 70-minute traffic delay per km for the users of that particular highway segment, while this effect is 160 minutes in recurrently congested segments.

While the car is a highly prominent mode of transport worldwide, walking remains an especially prevalent option in Europe's cities. Of all journeys undertaken, 20-40 percent are done so on foot or by bicycle, the highest percentage in Europe being recorded in the Netherlands. However, the economics literature has dedicated almost no interest whatsoever to walking.

One of the main reasons why people choose to live in a city is the presence of a rich variety of consumer goods and services in close proximity (Glaeser et al., 2001). In European city centres, shops are mainly concentrated in pedestrianised shopping streets and people can stroll around at their leisure as they window shop. By way of illustration, walking is such an intimate part of shopping that the majority of all Dutch pedestrian movements occur while shopping (Statistics Netherlands).

In retail markets, transportation costs are usually paid by customers and incurred on a shopping trip basis (Claycombe, 1991). Consumers who visit several shops during the same shopping trip ('trip-chain') benefit from reductions in transport (walking) and search costs. The associated reductions in costs for consumers imply a shopping externality for shops, which is enhanced when multiple shops are located in close proximity (Eaton and Lipsey, 1982; Claycombe, 1991; Schulz and Stahl, 1996).

In the current literature on retail location choices, there is a tendency to focus on spatial competition and on spatial or product differentiation (D'Aspremont et al., 1979; Osborne and Pitchik, 1987). There is also another growing line in the litera-

[6]'Rubbernecking' is the habit that road users, driving in the opposite direction to an accident, have of slowing down and craning their necks in order to view the aftermath of the accident.

ture that studies the impact of, above all, Wal-Mart on the retail market (Jia, 2008; Arcidiacono et al., 2016), among others on incumbent (discount) supermarkets and small grocery stores. However, the empirical literature has paid only limited attention to the importance of shopping externalities. While I am not the first to argue that the main reason why shops tend to cluster is the presence of shopping externalities, to the best of my knowledge, this is the first paper that quantifies these externalities.

This chapter makes several contributions to the literature. First, footfall — the daily number of pedestrians that pass by a shop — is a new, unique measure of shopping externalities. As I argue in this chapter, footfall has certain advantages over the standard density measures used in studies of agglomeration economies. In contrast to the extensive retail literature that focuses on US shopping malls (Brueckner, 1993; Pashigian and Gould, 1998; Konishi and Sandfort, 2003), I focus on the full population of the main shopping streets of the Netherlands. A key feature of these shopping streets is that they are dominated by two sectors: clothing and cafés/restaurants., both of which are known for offering highly heterogeneous products. This contrasts sharply with other retail sectors examined in the economics literature (e.g. movie theatres, gas stations, and video retailers, see Davis (2006); Netz and Taylor (2002) and Seim (2006)). Moreover, in contrast to the evidence for shopping malls, property ownership in the shopping streets under analysis is highly fragmented. As a consequence, there is no internalisation of shopping externalities in shopping streets and, thus, policies that foster retail concentration by providing subsidies are potentially welfare improving.

Finally, the main contribution of this chapter is the identification of shopping externalities by estimating the causal effect of footfall on the rental income of store owners, which depends on the rent paid by tenants as well as the probability of a property lying empty. As has been widely discussed in the agglomeration literature, proxies for spatial concentration, such as footfall, tend to be endogenous because they are correlated to unobserved location characteristics. We address this issue by focusing on shops that are located very close to each other (within 50m) but on different intersecting streets, controlling for an extensive set of shop and street characteristics.

Chapter 5 uses geo-located data of retail rents, shop vacancies and footfall in the Netherlands to quantify shopping externalities. First, a theoretical model formalises the existence of vacancies in the property market and establishes the relationship between shop rents and footfall, as well between vacancies and footfall. Identification is obtained using a novel research design based on spatial differences of footfall between intersecting shopping streets. The estimates imply an elasticity of rental income with respect to footfall of about 0.25 and about 0.1 with respect to the number of shops. The latter is substantial compared to the elasticities in the agglomeration economies literature. A shop's marginal benefit of a pedestrian passing by is about €0.004. The study also shows that footfall reduces shop vacancy rates considerably. Using the estimated elasticity of rental income, welfare considerations can be made taking into account new and existing shops. An average annual subsidy of about 10 percent of the rent to a new shop is welfare optimal, but when subsidies are given to existing shops, subsidies to shops that generate more footfall should be substantially higher.

The implications of these findings contribute to the ongoing policy debate on the decline of city centres in some European countries and the rise of large 'big-box' stores near the urban fringe (Sánchez Vidal, 2016). The study also complements the literature that demonstrates that the welfare effects of current planning policies hindering entry, especially that of large retailers, into retail markets, are negative. Indeed, several studies have shown that regulation policies reduce retail productivity and job growth and increase the market power of incumbent stores (Bertrand and Kramarz, 2002; Schivardi and Viviano, 2011; Haskel and Sadun, 2012; Cheshire et al., 2015).

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2 Express delivery to the suburbs. Highways in Europe's historical cities [§]

2.1 Introduction

Urban sprawl has been labelled 'a threat to the very culture of Europe' because of its impacts on the environment, on the social structure and on the economy (EEA, 2006). Controlling urban sprawl and suburbanization was one of the earliest reasons for the emergence of modern urban planning in Europe[1]. Already by the end of the 1920s in Britain, there was growing concern and opposition to the unprecedented scale and extent of suburbanization that seemed to be affecting every city in the country (Couch et al., 2008). Nowadays, the continuing growth of urban populations together with the new dynamics of immigration creates additional challenges in order to maintain or recover Europe's compact city shape.

Europe presents a series of unique characteristics that make it particularly interesting to study the effect of transport infrastructure on suburbanization. According to the OECD (2011), the average urban population density of the European metropolitan areas was 718 persons per km^2, compared to just 282 in the US. While European cities seem to be rather compact compared to most US cities, suburbanization is a reality in Europe. The average growth rate of population in the period 1961-2011 was 27 percent higher in the suburbs, compared to the central cities. However, the social class basis in US suburbs is different. In Europe, upper- and middle-class households live in the centre, as opposed to the US (Glaeser et al., 2008). This difference in the dominant urban spatial structure between Europe and the US has been explained by the difference in endowments of historical and other

[§] The paper in this chapter is coauthored with Miquel-Àngel Garcia-López and Elisabet Viladecans-Marsal. The title of this paper is inspired by the fact that the modern highway system that facilitates the 'express delivery' of goods and people to and from the suburbs has followed the routes of the main postal network that ensured the rapid delivery of mail in 1810.

[1] Urban sprawl refers to the expansion of a city's area while suburbanization to the relocation of population towards the outskirts.

urban amenities in the city centres of Europe (Brueckner et al., 1999). The importance of history on urbanization is also highlighted by the recent paper of Michaels and Rauch (2016). The authors use the different timing of the collapse of the Western Roman Empire in France and in Britain to conclude that history trapped many French towns in suboptimal locations. In the same line, Bosker and Buringh (2017) highlight the historical importance of physical geography as a major determinant of the modern system of cities in Europe.

Moreover, car use in Europe is about 42 percent lower than in the US (Eurostat and OECD, 2011), while public transportation flows, in particular rail passenger transport, are much higher in the EU than in the US (391.8 vs. 10.3 billion passenger-km in 2015) (International Union of Railways, 2015). Although car use in Europe is argued to be relatively low, the highway network grew inmensely during the second half of the 20th century, from 259 km in 1955 to 67,779 km in 2011. Much of this development was financed by the EU Regional and Cohesion Funds[2]. At the same time, EU policies have sought to mitigate the problems that the literature has identified as the potential repercussions of suburbanization and urban sprawl i.e. CO_2 emissions, energy inefficiency (Glaeser and Kahn, 2010) and social segregation[3] (Glaeser and Kahn, 2004). Nonetheless, the expansion of the highway network should not be considered in isolation in Europe, given the prominent role played also by the continent's railways since the 19th century. Indeed, the share of railroads has recently increased, reflecting EU objectives for a *Single European Railway Area* (European Commission, 2010).

European cities are very heterogeneous. Many big cities in Europe thrived as Roman or Medieval cities while others emerged during or after the Indrustrial Revolution. Countries and regions in Europe have also followed different development and urbanization paths during the late twentieth century. Suburbanization spread from Northern to Southern European cities and from the largest to the medium-sized ones. Southern European cities experienced 'urbanisation without industrialisation' and informal job growth, while popular land colonisation expanded the suburbs (Leontidou, 1990). The formerly planned Eastern European countries were

[2]During the first 15 years of its existence, the European Regional Development Fund devoted 80 percent of its funding to infrastructure projects (Vickerman, 1991) and in the period 2000-2006 about 35 percent of the Structural Funds and 50 percent of the Cohesion Fund were spent on infrastructure projects (Crescenzi and Rodríguez-Pose, 2012). During the period 2007-2013, again, approximately 35 percent of the total amount spent by the Structural and Cohesion Funds was invested in roads, mainly highways (DG-REGIO, 2016).

[3]*Europe 2020* strategy focuses on reducing CO_2 emissions and increasing energy efficiency; fighting social exclusion; and promoting education and R&D. Although the last two areas might seem to be irrelevant to this discussion, they reflect typical criticisms levelled at the allocation of EU funding, often believed to favour 'hard infrastructure' (e.g. highways) as opposed to 'soft infrastructure' (e.g. human capital) investments.

exposed to the market forces that demanded a redistribution of urban population and transport infrastructure improvements during the transition period. Finally, except for the heterogeneity between countries, there is evidence of a substantial breakup of the previous regular pattern of decentralisation. During the 1980s, there was a significant degree of recentralisation in many Northern European cities. "The pattern is that there is now a greater variation in patterns" (Cheshire, 1995).

While many detailed studies of small areas have been reported, few examine broad cross-sections of cities and even fewer turn their attention to analyse cities across various countries. One of the main problems impeding such analyses at the European level is the lack of harmonized urban data. In this paper, we are able to overcome this problem by creating most of the variables used in our analysis from maps. Using historical transportation in Europe as an instrument, we estimate the *joint causal* effects of highway and railway infrastructure on the suburbanization for 579 cities in 29 European countries during the period 1961-2011. To the best of our knowledge, these effects have never been studied before and certainly not at this scale. Yet, the impact of transport infrastructure improvements on urban spatial structure is a major concern for Europe.

Our main results are in line with the related literature. Specifically, we find that an additional highway 'ray' displaced on average approximately 9 percent of central city population in European cities during the period 1961-2011, while we find no significant effect of the railways. Previous studies for the US (Baum-Snow, 2007a) and Spain (Garcia-López et al., 2015) estimated the causal effect of highway 'rays' on suburbanization at 9-12 and 8-9 percent, respectively, while the same effect was estimated at 4 percent for China (Baum-Snow et al., 2017). The latter study also found that ring roads displaced an additional 20 percent of central city population, while they found no effects of railways on suburbanization. On the other hand, Garcia-López et al. (2017) study the effect of the Regional Express Rail (RER) in the metropolitan area of Paris and find that each kilometre closer to a station increases employment and population growth by 8 and 12 percent, respectively.

In order to tackle the problem of endogeneity, we extend the standard instrumental variables (IV) in a long-difference specification by employing panel data methods, using city fixed effects and regional-specific time fixed effects in addition to the IV two-step approach. We take advantage of the rich history of Europe, which is reflected in the number of different types of transport infrastructure employed since the Romans built their roads more than 2,000 years ago[4]. In particular, the main postal routes in 1810 and the railways in 1870 may explain the topology of

[4]The historical transport variables that have actually been tested in this study as potentially valid instruments are the Roman roads, the main trade routes in the Holy Roman Empire and neighbouring countries in the 15th century, the main and secondary postal routes in 1810 and the railways in 1870.

the modern transport network, while being exogenous to modern suburbanization.

While the findings of this paper confirm the causal relation between the highway infrastructure and suburbanization reported in the literature, we find evidence of an heterogeneous effect of transport on suburbanization. When employing the full time span covered by our data, we find that the effect of highways and railways on suburbanization varies significantly with the period of time under consideration. Specifically, the estimated effect of highways on suburbanization was significantly higher during the period 1961-1981 than it was during the more recent decades. In addition, railways seem to have also contributed to suburbanization mainly during this first two decades. Moreover, apart from the radial variables, we also include the nodes of the two networks (highway ramps and railway stations) to account for the accessibility to the transport infrastructure network. We find evidence that the effects of highways on suburbanization cannot be solely attributed to the radial nature of the networks.

A number of other interesting findings emerge from the heterogeneity of European cities. By exploiting this heterogeneity, we test whether the effects of transport infrastructure on suburbanization vary when cities with different size, history or geography are considered. Specifically, we observe a pattern indicating that highways caused less suburbanization in the cities with 'more history'. Brueckner et al. (1999) and Koster et al. (2016) report evidence of the importance of historical urban amenities in European central cities, which further supports our results. This finding is highly related to a growing literature on the importance of consumer amenities in a city (Glaeser et al., 2001; Carlino and Saiz, 2008; Lee and Lin, 2017; Koster et al., 2016), as well as the paper of Brinkman and Lee (2016) who highlight the disamenity effects of highways on city centres and their relevance with the freeway revolts that spread after 1955 in the US.

Finally, we attempt to estimate the impact of European regional policies on suburbanization. However, we do not find a significant effect of the latter on suburbanization. This finding indicates that the highway investments made by the EU Regional and Cohesion Funds were not responsible for promoting the suburbanization of receptor cities on average.

The rest of this paper is organized in four sections and three Appendices. Section 2.2 describes the process of database construction and presents some descriptive statistics about suburbanization and the evolution of the transport network in Europe. In Section 2.3, we discuss our identification strategy and we present our first- and second-stage results. In Section 2.4 we present heterogeneous estimates of the effect of transport infrastructure on suburbanization when we divide our sample of cities according to the time period considered, their size, history and geographical area. In Section 2.5, we highlight the most important findings and we draw our final conclusions. Finally, 2.6.1 includes the maps that are discussed in the main text, 2.6.2 presents some additional robustness checks and 2.6.3 presents some additional heterogeneous results based on the natural geography of cities.

2.2 Suburbanization and transportation in Europe

2.2.1 Database construction

Apart from the population data, all the data that have been used in this paper are derived from maps using GIS software. Although this task involved a considerable amount of map processing (including geo-referencing, map vectorizations and manual network editing), this data collection strategy allowed us to focus on the city level for the whole of Europe and for a long period of time.

The urban population dataset employed in this paper was constructed using census population figures collected every 10 years at the municipal level for the period 1961-2011 in 34 European countries, as provided by the DG REGIO of the European Commission. In our analysis, we use 29 countries for which complete data were available and that Eurostat includes in its Urban Audit. The countries included in our dataset are the member-states of EU28 member states (with the exception of Slovenia and Lithuania, for which data were not available) and three non-EU countries, Switzerland, Norway and Iceland. To the best of our knowledge, this is the first time that this new integrated census population dataset has been used in an empirical study.

The units of our analysis are the *Core Cities* (*CCs*)[5] and the *Large Urban Zones* (*LUZs*) as defined by Eurostat in the 2008 Urban Audit[6]. Eurostat defines LUZs not only in terms of their administrative and statistical unit borders but also in relation to commuting criteria, defining a functional urban area based on a perfectly harmonised methodology across Europe[7]. This definition comprises all the settlements that interact economically with the core (Arribas-Bel et al., 2011). Thus, Eurostat's LUZs were chosen as the most appropriate spatial unit for the analysis of suburbanization in Europe. The Urban Audit uses the concept of the CC as a

[5]In this paper our use of the term *central cities* is synonymous with that of core cities.

[6]For London and Paris, which are by far the biggest cities in our sample, we use Eurostat's *Kernel* definition (created when the urban centre stretches far beyond its boundaries (Eurostat, 2014)) since in these cases their CC area is extremely small with respect to that of their LUZ area (0.04 and 0.8 percent respectively) and it does not reflect the actual extent of their CBD.

[7]Eurostat's LUZs approximate the *Functional Urban Area* (*FUA*) as defined by the OECD. The OECD and the European Commission developed a new harmonized definition of a city and its commuting zone in 2011. This new OECD-EC definition identified more than 800 cities with an urban centre of at least 50,000 inhabitants in the EU, Switzerland, Croatia, Iceland and Norway.

legal, administrative entity and defines it in relation to its political/administrative boundaries.

In spite of being one of the most solid and comprehensive statistical datasets available at the city level in Europe, the Urban Audit suffers from many missing values (even in the city population series), which means many of its variables are unsuitable for use. For this reason, we only adopt the delineation of the LUZ and the CC areas, and use census data at the municipal level to construct our LUZ and CC population dataset. This was a challenging task as it meant retrieving information for the numerous municipal mergers and changes in municipal codes from the national statistical offices. Our final dataset comprises 579 LUZs, each consisting of a CC and a suburban area, for the period 1961-2011.

The transport infrastructure measures that we use in this paper were calculated using GIS maps of the road system and the railroad network in Europe that form part of the *RRG* GIS Database[8]. The highway and railway definitions used in this dataset follow their corresponding country definitions. RRG constructed the highway and railway network in each decade in the period 1961-2011. Using the 2011 operational networks as their starting point, they went back in time, decade by decade and they deleted all the highway and railway segments that were not constructed in each of the previous decades. From the resulting digital maps, we calculated the number of highway and railway 'rays', in line with Baum-Snow (2007a) definition, as "limited access highways connecting the central city to a significant part of the suburbs". Finally, the RRG GIS Database also provides information for highway ramps and train stations[9].

We also calculated an alternative measure of the number of radial highways and railways by modifying the algorithm used for counting rays developed in Baum-Snow et al. (2017). In our version, we use the CC 'smoothed' *area* as opposed to the CBD *point* in Baum-Snow (2007a). To construct the smoothed areas, first, we buffered out and in the CC border using a 5-km radius in order to eliminate any irregularities in the shape of the CC area that might result in a spurious count of the intersecting highways. Then we used a buffer ring of 5-km radius, clipped in order to match the borders of the LUZs (should the ring extend beyond its borders). We then excluded the intersection points that coincided in both the ring and the smoothed CC. Finally, we define the number of algorithm rays for any given city as the minimum number of highway intersection points between the smoothed CC border and the 5-km buffer ring. Although this method provides an alternative ray

[8]Büro für Raumforschung, Raumplanung und Geoinformation (RRG) GIS Database.

[9]It should be mentioned that we have excluded the high-speed rail lines since they were built in order to connect different cities. High-speed trains make very few stops and hence, they cannot facilitate intrametropolitan commuting.

measure, the manual count of rays is more accurate. We argue that this is the case since our algorithm overcounts the number of railroad rays. This is because in our GIS dataset, there are often parallel lines of rail following an identical path. Such rays are counted as two rays while in reality they facilitate commuting only from one part of the LUZ to the CC[10]. Additionally, highway rays are also undercounted since in many European cities highways do not penetrate the inner central cities (Cox et al., 2008) or they continue as main roads (based on our GIS data) inside the CC.

To compute our historical instruments, we worked with two digital vector maps. For the 1810 postal routes and for the 1870 railroads, we created our own GIS maps by geo-referencing and vectorizing the scanned map from the David Rumsey Historical Map Collection[11] and the map from the Historical GIS for European Integration Studies[12], respectively. To calculate the number of these historical transport infrastructure rays, we adopted the same definition as that used above for the highways and railways.

We also include a number of historical variables in our analysis. The main historical variables used are dummy variables for the Roman cities, Medieval cities, major cities in 1000 and 1450[13] and the population in 1850 (Bairoch et al., 1988)[14]. In addition, we created dummy variables for the cities with universities between the 12$^{\text{th}}$ and the 15$^{\text{th}}$ centuries, cities with Roman settlements and cities with bishoprics (in 600, 1000 and 1450) from the maps in the Digital Atlas of Roman and Medieval Civilization. We also created dummy variables for cities with medieval monasteries and for cities with a historical city centre or another landmark recognized by UNESCO.

In addition, we used a number of geographical variables, namely mean elevation, altitude range and the Riley et al. (1999)'s index of terrain ruggedness for each CC and each LUZ[15]. Another important geographical variable is the distance separating each LUZ centroid from the closest coastline. Finally, we use raster GIS temperature data for 0.86 km^2 cells from `http://www.worldclim.org/tiles.php?`

[10]It should be borne in mind that our measure of rays does not include any individual rays characteristics. Nevertheless, any such characteristics that are time invariant should be controlled by the LUZ fixed effects.

[11]See `http://www.davidrumseny.com`.

[12]HGISE, see `http://www.europa.udl.cat/hgise`.

[13]We created these variables from the Digital Atlas of Roman and Medieval Civilization (DARMC).

[14]The European cities included in this dataset are those that had 5,000 or more inhabitants at any point between the 8$^{\text{th}}$ and the 18$^{\text{th}}$ centuries. For 1850, we have information regarding the exact population of these cities.

[15]The original GIS raster maps were downloaded from the Digital Elevation Model over Europe; see `http://www.eea.europa.eu/data-and-maps/data/eu-dem`.

`Zone=16` and data on navigable rivers from `https://www.evl.uic.edu/pape/data/WDB/`.

2.2.2 Patterns of suburbanization in Europe

In this section, we present some descriptive statistics of the population in the central cities and in the suburbs of the LUZs included in our sample to illustrate the patterns of suburbanization in Europe. We define the degree of *relative urbanization/suburbanization*[16] as the difference between population growth in the CC and population growth in the suburbs. Positive differences indicate urbanization and negative differences, suburbanization. As can be observed in the last row of the last column of Table 2.1, on average, European cities experienced suburbanization in the period 1961-2011. Moreover, the degree of suburbanization did not vary substantially over time but remained relatively stable throughout the whole period of study. However, in the decade 1961-1971, the growth in city population was by far the highest in the whole period.

Table 2.1: Average population growth and (sub)urbanization

	1961-1971	1971-1981	1981-1991	1991-2001	2001-2011	1961-2011
Population Growth (LUZ)	12.29%	6.69%	3.66%	3.07%	5.29%	**34.77%**
(*i*) *CC* Pop. Growth	10.83%	4.23%	1.72%	0.13%	4.22%	**22.62%**
(*ii*) Sub. Pop. Growth	14.08%	7.49%	7.95%	6.25%	6.38%	**49.61%**
Relative (Sub)urbanization	**-3.26%**	**-3.26%**	**-6.22%**	**-6.11%**	**-2.16%**	**-26.99%**

Notes: Relative (sub)urbanization is the difference between (*i*) and (*ii*). Positive values indicate relative urbanization and negative, relative suburbanization.
Source: Authors' own calculations based on data from DG REGIO (EC)

Table 2.1 indicates that suburbanization was, on aggregate, the dominant process in Europe, with 299 of the 579 urban centres (roughly 50%) in our analysis experiencing suburbanization during the period 1961-2011. This is partly explained in Table 2.2. The last column of this table shows that the overall suburbanization pattern (as highlighted in Table 2.1) was driven mainly by the population displacement in Europe's biggest cities (4th quartile). In contrast, small and medium-small cities (1st and 2nd quartile) experienced intense urbanization during the first few decades but underwent a process of suburbanization in the last two decades of our sample. On the other hand, medium-big (3rd quartile) cities experienced moderate suburbanization on average, while the most intense suburbanization was recorded in the big cities (4th quartile).

[16]Urbanization/suburbanization hereafter.

Another useful descriptive measure of the pattern of suburbanization in Europe can be obtained from Map 2.1. The cities in Eastern Europe and in the Mediterranean countries experienced significant urbanization in those years that the cities in Western Europe suburbanized. This heterogeneous pattern of urbanization/suburbanization presented by cities of different sizes and from different geographical locations motivated the heterogeneous estimations that we present in Section 2.4.4.

Table 2.2: Quartile city size (sub)urbanization by decade

City size quartiles	1961-1971	1971-1981	1981-1991	1991-2001	2001-2011	1961-2011
1st (23,892 - 111,673)	27.84%	18.30%	7.88%	-5.00%	-5.47%	**62.14%**
2nd (111,674 - 178,017)	15.99%	6.89%	2.77%	-5.36%	-5.15%	**17.69%**
3rd (178,018 - 343,067)	7.01%	4.51%	-3.49%	-6.33%	-3.71%	**-3.35%**
4th (343,067 - 10,618,868)	-10.36%	-11.58%	-6.69%	-6.45%	-1.19%	**-44.36%**

Notes: City size quartiles were calculated based on 1961 LUZ population.
Source: Authors' own calculations based on data from DG REGIO (EC)

2.2.3 European transport infrastructure: Origins and evolution

The origins of Europe's modern transport infrastructure can be traced to the Roman era, before which the continent's roads were of a distinctly local nature, being used to facilitate short distance journeys. The Romans were the first to build an extensive and sophisticated network of paved and crowned roads, designed to meet military and commercial goals. Overall, they built more than 85,000 km of main roads, which radiated out from Rome, linking up the different territories in its Empire, from Britain to Syria (O'Flaherty, 1996). Other important ancient roads of note included the *amber routes*, which connected the northern European sea-shores with the Adriatic Sea during the Bronze Age, and in the 15th century, the *main trade routes in the Holy Roman Empire* and neighbouring countries that linked up various centres of commerce in Central and Northern Europe with Instanbul.

Although there have been roads in Europe since ancient times, they only became popular a few centuries ago. At the beginning of the 17th century, the continent's governments realized that an improved road system could foster economic prosperity and better governance and that roads could facilitate the creation of a reliable postal system. Postal road systems were thus developed throughout Europe during the 17th and 18th centuries. While postal routes were relatively primitive until the middle of the 18th century, in the last quarter of that century, the improvement in road construction, including the introduction of hard surfaces and the development of much improved carriages, permitted the use of wheeled coaches and wagons, which in turn led to the development of coach services between towns. These coaches were provided primarily by the public mail service which was designed to carry letters, packages, and people. Indeed, until the 19th century, most passenger coach travel was monopolized by the postal carriers. These improvements resulted in a significant increase in road traffic, ushering in the so-called 'mail coach era', which lasted until the middle of the 19th century, when railroads became the primary mode of transportation (Elias, 1981, 1982).

The postal route network can be regarded as the precursor of Europe's modern intercity road network. Due to its earlier popularity and Europe's rugged landscape, modern highways have tended to follow its path. However, almost no 19th-century postal routes have been preserved to the present day. Map 2.2 and Table 2.3 depict the evolution of the highway network in Europe between 1961 and 2011[17]. In 1961, there were very few highways concentrated in a handful of countries[18]. However, during the sixties, Europe's highway network grew enormously. By 2011, the highway network had expanded across the whole European continent. The fact that in 1961 the highway network in Europe had hardly developed allows us to use this year as the starting point for its subsequent evolution.

Table 2.3: The evolution of the highway and railway network in Europe.

Year	Highway length (km)	Railway length (km)
1955	259	297,942
1970	15,036	269,659
1980	28,213	260,464
1990	43,502	235,263
2000	57,763	217,324
2010	67,779	225,333

Notes: The highway length statistics refer to the EU28 countries (except for Greece), as well as Norway, Switzerland, Turkey and the Former Yugoslav Republic of Macedonia. The railway length statistics refer to the EU15 countries (except for Luxembourg) as well as Hungary, Norway, Poland, Romania, Switzerland and the Former Yugoslavia countries.
Source: Eurostat (highways) and Atlas on European Communications and Transport Infrastructures and RRG dataset (railways)

The prominent role played by highway infrastructure in Europe is clear from Map 2.2. However, we should not neglect the other main transport infrastructure, namely the railroads. The development of Europe's rail network can be divided in four stages: initial expansion (1840-1860), general expansion (1860-1910), stabilisation (1910-1960) and contraction (1960-2010) (Martí-Henneberg, 2013). Until 1860, Europe's railway network in Europe was very sparse and only in the UK had

[17]The highway and railway datasets included in our empirical analysis were only constructed for the metropolitan areas in our sample. To show the evolution of the whole transport network, we use data at the country level.

[18]Primarily in Germany, the Netherlands, some in Northern Italy and very few in Belgium, Croatia and Poland.

the network acquired any degree of density. However, by 1870, the railroads had expanded across the whole continent and the importance of Europe's railway network was well established.

As can be seen in Map 2.3 and Table 2.3, railroads linked up much of Europe by 1870. However, during the following century, the railway network expanded to virtually every corner of the continent and its density increased enormously. In the period 1870-1900, numerous lines were opened up. While many new lines continued to be created in the periods 1910-1960 and 1960-2010, many lines were also closed down. Most of these railway closures occurred in Western Europe, where the 1870 railway network had been denser and they were typically attributable to underlying political factors[19]. The large number of line closures, together with the inauguration of many new lines, suggests that the rail network changed radically between 1870 and the decades from 1960 to 2010. These circumstances support the use of this initial expansion of the railroad network in 1870 as an exogenous instrument for the modern railroad network.

2.3 Effects of transportation on urban structure

2.3.1 Identification

The classical monocentric land use theory developed by Alonso (1964), Mills (1967) and Muth (1969) predicts that the declining transport costs push some people away from the city core, thus lowering population densities in city centres. Wheaton (1974) shows that higher metropolitan population leads to an expansion of the metropolitan boundary and rising densities throughout the city without any modification to the rent and density gradients of the open city system. The combined impact of population growth and the effects of transportation causes a flattening of rent and density gradients, while rents and population density increase in the suburbs. Based on this extension of the basic monocentric model and on the model of radial commuting highways proposed by Baum-Snow (2007b), we estimate the effect of highway rays, highway ramps, railway rays and railway stations on central city population. We measure the effect of transportation infrastructure on suburbanization indirectly by using the LUZ population as a control variable.

Concerns about endogeneity in this estimation have already been discussed in the associated literature (Baum-Snow, 2007a; Duranton and Turner, 2012; Garcia-López et al., 2015). Here, a main issue is the simultaneous causality bias between

[19]For example, the Federal Republic of Germany rationalized its railway network after the large-scale expansion during the Third Reich (Mitchell, 2006), while the Democratic Republic of Germany decided to maintain its public sector infrastructure.

the transport infrastructure variables and population change in the CC. As argued in the literature, it is not only highways than can impact central city populations, but a city's prospects for growth or decline can also affect the policies regarding the allocation of new lines of transport infrastructure in that cities. Another endogeneity issue might arise owing to the fact that unobservable factors can cause omitted variable bias in an OLS specification. Here, it is clear that a city's past and recent economic growth can affect both the CC's population change and the allocation of transport infrastructure.

In European cities, the bias introduced by both these concerns could be either positive or negative. On the one hand, more transport infrastructure investments have typically been allocated to the more thriving urban areas, in terms of population or income. On the other hand, EU Regional and Cohesion Policies (and even some national policies) have targeted the lagging regions and cities in order to promote their growth potential and convergence with the rest of the EU.

To obtain an estimate of the causal effect of transport infrastructure improvements on CC population growth, we employ two-stage least square (TSLS) regressions using the exogenous variation provided by the historical transport infrastructure measures, which we use as instrumental variables (IV). However, using panel data IV requires an instrument that varies over time. To this end, we adopt a 'shift-share' (Bartik, 1991) approach using 'smoothed' instruments, similar to the 'smoothed rays in the plan' instrument in Baum-Snow (2007a).

Smoothed postal route rays are calculated by multiplying the number of postal route rays in 1810 by the fraction of the highway mileage in each country completed at each point in time[20]. The postal route rays' instrument can be thought of as the segments of the 1810 postal route rays that would have been completed in every decade had the postal route network followed the same rate of evolution of the modern highway network (length) in each country. The same process is followed to calculate the smoothed radial railways in 1870. Finally, by the same token, we have applied this methodology for the postal route and the 1870 rail *length* variables, which we use as instruments for the highway ramps and the railway stations, respectively.

While the related literature has focused mainly on long-difference specifications, we use panel specifications that allow us to control for unobservable city characteristics and for regional-specific time fixed effects. By using regional-specific time fixed effects, we control for changes in the CC population that are decade-specific

[20]The country highway and railway mileage at every decade is the sum of the mileage for all LUZ in each country. However, using the fraction of mileage in the whole of Europe, our main results continue to hold.

for the cities of the same NUTS1 region[21]. These interaction dummy variables, together with the LUZ fixed effects and the exogenous variation provided by our instruments, constitute the identification strategy employed in this paper.

An important innovation made by this paper is the fact that we do not only estimate the effect of each type of transport infrastructure individually, but we also estimate the joint effects of different transport infrastructure types and measures instrumenting all the transportation variables.

$$\begin{aligned} ln(Pop_{it}^{CC}) = \beta_0 + \sum \beta_1 \widehat{Transport}_{it} + \beta_2 ln(Pop_{it}^{LUZ}) + \\ + \vartheta^{LUZ} + \vartheta^{t} * \vartheta^{NUTS1} + \nu_{it} \end{aligned} \tag{2.1}$$

Equation (2.1) is the second-stage specification in which we regress the logarithm of the population that lives in the CC of city *i* in year *t*, $ln(Pop_{it}^{CC})$, on the highway and railway variables, $\widehat{Transport}_{it}$, controlling for the logarithm of the LUZ population, $ln(POP_{it}^{LUZ})$. The reason why we use the summation symbol before $\widehat{Transport}_{it}$ is because, in addition to individual effects, we also estimate the joint effects of different transport infrastructure measures. Finally, ϑ^{LUZ}, ϑ^{t} and ϑ^{NUTS1} stand for LUZ, decade and NUTS1 regional dummies, respectively. Standard errors are clustered by NUTS3 regions. However, in Section 2.6.2 in the Appendix, we also cluster the standard errors by NUTS1 regions in order to control for intraregional city interaction effects.

$$\begin{aligned} \widehat{Transport}_{it} = \alpha_0 + \sum \alpha_1 Historical\ transport_{it} + \alpha_2 ln(Pop_{it}^{LUZ}) + \\ + \eta^{LUZ} + \eta^{t} * \eta^{NUTS1} + \epsilon_{it} \end{aligned} \tag{2.2}$$

Equation (2.2) presents a general form of the first-stage specification, where $\widehat{Transport}_{it}$ includes highway rays, highway ramps, railway rays or railway stations. $\sum \alpha_1 Historical\ transport_{it}$ are the historical transportation variables that are used as instruments in each specification. As discussed, we are able to estimate the joint effects of two different transportation infrastructure types or measures. As a result, instrumenting two independent variables means that the first-stage equation of each of these variables includes both instruments[22].

[21] On average, there are 6.2 cities in each NUTS1 region.

[22] We always use the same number of instrumented variables and instruments (equations are exactly identified).

2.3.2 First-stage results: History paved the way

In Section 2.2.3, we documented the history and evolution of Europe's modern transport infrastructure. Accordingly, it seems that Europe's highway network has followed the routes taken by its historical postal network in 1810, while the modern railway network has expanded adhering to the first extension of the continent's railways in 1870. In addition, it is our contention that it is unlikely that these two historical transportation systems directly affected the population of European central cities during the second half of the 20th and the beginning of the 21st centuries, providing intuitive evidence that the postal routes in 1810 and the railways in 1870 satisfy the assumption of instrument exogeneity and that of instrument relevance. In this section, we present the first-stage panel estimates, which empirically show that the postal routes in 1810 and the railways in 1870 are relevant instruments for the modern highway and railway networks, respectively.

Table 2.4: Modern and historical transport infrastructure: First stage results

	Decade variables			
Dependent variable:	Highw. rays	ln(sub. ramps)	Railw. rays	ln(sub. stations)
	OLS [1]	OLS [2]	OLS [3]	OLS [4]
1810 smoothed postal route rays	0.315^a (0.037)			
ln(1810 smoothed postal route km)		0.258^a (0.016)		
1870 smoothed railroad rays			0.589^a (0.116)	
ln(1870 smoothed railroad km)				0.419^a (0.031)
ln(LUZ population)	✓	✓	✓	✓
NUTS1-specific year fixed effects	✓	✓	✓	✓
LUZ FE	✓	✓	✓	✓
Adj. R^2	0.660	0.710	0.696	0.719
Observations	3,474	3,474	3,474	3,474

Notes: The estimates presented in Columns [1]-[4] include 579 cities in 6 decades (1961-2011). The historical transport variables are smoothed; i.e. they are time varying and they are computed by multiplying the number of historical rays/length by the fraction of the highway/railway mileage in each country completed at each decade. Robust standard errors are clustered by NUTS3 regions and are in parenthesis. ^{a}, ^{b} and ^{c} indicates significant at 1, 5, and 10 percent level, respectively.

Table 3.3 includes the first-stage results of our panel estimates. All these panel specifications include the logarithm of the LUZ population, LUZ fixed effects, as well as NUTS1-specific year fixed effects[23]. Columns [1] and [2] show the first-

[23] This is the interaction of the 97 NUTS1 regional dummies with the six decade (year) dummies.

stage results for the highway ray and ramp variables, respectively. As can be seen, the smoothed postal route rays that we use as an instrument for the number of highway rays in each decade is highly statistically significant and positive. The same holds for the logarithm of the suburban postal route length as an explanatory variable of the logarithm of highway ramps. The railway results presented in Columns [3] and [4] are no different. We calculated the logarithms of all the length and node measures and added one unit (metre in the case of length) to each observation in order to avoid omitting the observation with zero values.

In order to validate the relevance of our instruments, Table 2.12 in Section 2.6.2 shows the first-stage of a long-difference specification that includes a number of historical and geographical control variables. Table 2.12 confirms the relevance of our instruments after controlling for the role of history and geography.

2.3.3 Second-stage results: The 'drivers' of suburbanization

Table 2.5 shows our main average results when estimating equation (2.1) for the whole sample of cities. Column [1] shows the results of a simple OLS regression in which we estimate the joint effect of highway and railway rays on suburbanization. The highway ray coefficient appears to be highly statistically significant and negative while the railroad ray coefficient is essentially zero. However, as discussed above in Section 2.3.1 and in the literature, this OLS regression might be biased. In order to confirm and avoid this bias, the results of Columns [2]-[9] are estimated using TSLS using the postal routes and the railways in 1870 as instrumental variables for the modern highway and railway network, respectively.

Column [2] shows the results of the TSLS regression when we use the highway rays as our main variable of interest. The estimated highway coefficient is highly statistically significant and its value is -0.089. This estimate is in line with the negative effect of highways on CC population that has been found in the related literature (Baum-Snow, 2007a; Baum-Snow et al., 2017; Garcia-López et al., 2015). In addition, the value of our estimated highway coefficient is significantly higher than the OLS regression of Column [1]. We believe that OLS underestimates the effect of highways on suburbanization because many of the highways in Europe were allocated to the poorer regions with smaller cities in order to promote the equity objectives of the EU Regional Policy or in order to increase the transnational EU connectivity.

Table 2.5: Main results

Dependent variable:	ln(Central city population)								
	OLS [1]	TSLS [2]	TSLS [3]	TSLS [4]	TSLS [5]	TSLS [6]	TSLS [7]	TSLS [8]	TSLS [9]
Highway rays	-0.031^{a}	-0.089^{a}		-0.107^{a}			-0.094^{a}		-0.054^{b}
	(0.005)	(0.017)		(0.021)			(0.018)		(0.021)
ln(suburban ramps)			-0.054^{a}	0.054^{b}				-0.061^{a}	-0.002
			(0.012)	(0.026)				(0.012)	(0.030)
Railroad rays	0.004				-0.076^{a}		-0.003	-0.045^{a}	-0.015
	(0.005)				(0.021)		(0.017)	(0.006)	(0.012)
ln(suburban stations)						0.007			
						(0.010)			
ln(LUZ population)	✓	✓	✓	✓	✓	✓	✓	✓	✓
LUZ fixed effects	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year×NUTS1 dummies	✓	✓	✓	✓	✓	✓	✓	✓	✓
First-Stage F-statistic		59.9	260	26.4	25.6	184.8	9.3	206.8	10.3
S. & Y. 10% critical values	-	16.4	16.4	7	16.4	16.4	7	7	-
Observations	3,474	3,474	3,474	3,474	3,474	3,474	3,474	3,474	3,474
Instruments:									
1810 postal route rays		✓		✓			✓	✓	✓
ln(1810 postal route km)			✓	✓					
1870 railroad rays					✓		✓	✓	✓
ln(1870 railroad km)						✓			

Notes: The estimates presented in Columns [1]-[9] include 579 cities in 6 decades (1961-2011). Our historical instruments are smoothed; i.e. they are time varying and they are computed by multiplying the number of historical rays/length by the fraction of the highway/railway mileage in each country completed at each decade. S. & Y. refer to Stock and Yogo (2005). Robust standard errors are clustered by NUTS3 regions and are in parenthesis. ^{a}, ^{b} and ^{c} indicates significant at 1, 5, and 10 percent level, respectively.

Column [3] includes the logarithm of suburban highway ramps as a measure of suburban highway accessibility as an alternative and complementary measure of highway infrastructure[24]. The coefficient for the highway ramps is highly significant and negative and its value is -0.054. Column [4] includes both highway rays and the logarithm of suburban ramps in order to separate CC highway penetration and the impact of suburban accessibility. It appears that when we include the two highway measures jointly, both are statistically significant, albeit the suburban ramps coefficient is positive. This positive coefficient of suburban ramps could be interpreted as an effect on urban growth that has been found in the related literature (Duranton and Turner, 2012). The results of Column [4] suggest that the distinct effects of the different measures of highway infrastructure cannot be eas-

[24]Suburban ramps and stations are very highly correlated with the total number of ramps and stations in the LUZ. However, suburban nodes are less correlated with the number of rays than the LUZ nodes. For this reason and in order to capture the accessibility to transport network in the suburbs, we chose to include the suburban counts of nodes instead of the LUZ counts.

ily disentangled. This is the main reason why we decided to show all informative specifications in all the result tables hereinafter.

The method used to select the specifications that we finally include in each output table is the following. First, we estimate individual specifications for both highway rays and highway ramps. If both coefficients are significantly different from zero, we estimate the joint effect of highway rays and ramps. We proceed in the same way for railways (rays and stations). Then, we estimate the joint highway-railway effect for all the couples (or triples) of jointly or individually statistically significant variables (if any). If, for example, highway rays are the unique statistically significant variable in a joint highway rays-ramps specification, we only include highway rays in the joint highways-railways specification (if any railway coefficients are statistically significant). If, on the other hand, none of the highway rays or ramps are statistically significant in the joint highway rays-ramps specification, we estimate the joint highway-railway specifications (again, if any railway measure is statistically significant) for both highway rays and ramps. It should be stressed that the first-stage F-statistic tests in Section 2.4 are not always above the Stock and Yogo (2005) 10 percent critical values. Nonetheless, for the sake of completeness and consistency, we prefer to show all the results and interpret them with caution when the instruments are not strong.

Columns [5] and [6] present the results for railway rays and stations, respectively. Column [5] indicates that the railway ray coefficient is also highly significant and negative. In addition, its value is similar to the value of the highway ray coefficient. Yet, Column [6] shows that in the case of railways, the measure of suburban accessibility (stations) is not statistically significant for suburbanization. Therefore, in accordance with our method for selecting the most meaningful specifications, we do not include a joint specification for the two rail measures[25].

In Column [7], both highway rays and radial railways are included. This specification suggests that when the two types of transport infrastructure rays are jointly considered, railways are not statistically significant, while the highway coefficient is hardly unchanged compared to the individual specification in Column [2]. The finding that the effect of railway rays on suburbanization is biased when railways are considered individually is crucial and highlights the importance of jointly considering highways and railways in the study of suburbanization. Column [8] includes the measure of suburban ramps together with railway rays. In this specification, railway rays seem to be statistically significant as well. However, in Column [9], where we include all highway rays, suburban ramps and suburban stations, it seems that when considered jointly, the effect of transport infrastructure on suburbanization can be

[25] In any case, the resulting output is approximately a reproduction of the railway ray and station coefficients and the standard errors from Columns [5] and [6].

attributed solely to highway rays.

We consider specifications [7] our preferred specifications while the specification in Column [9] is also very instructive. However, due to the complexity of a TSLS estimation with three instrumented variables and the correlation between the two highway measures, we prefer to interpret the results of Column [7]. Column [7] indicates that an additional highway ray displaced on average 9.4 percent of the European CC population in the period 1961-2011. This estimate is similar to the previous empirical findings in the related literature for the developed countries (Baum-Snow, 2007a; Garcia-López et al., 2015). However, in the following section (Section 2.4) we highlight the heterogeneity of this effect in terms of time period, history and geography.

2.3.4 Robustness checks

In Section 2.3.3, we argued that our preferred specification includes both highway and railway rays (Column [7] in Table 2.5). In this specification, we used TSLS with historical 'shift-share' instruments to avoid omitted variable bias and reverse causality bias. However, one could argue that the aforementioned specification might suffer from other sources of bias. In Table 2.6, we try to address any such concerns using different specifications following the one in Column [7]. We also present the same robustness checks for the final specification of Table 2.5 (Column [9]), Table 2.13 in Section 2.6.2[26].

Column [10] uses the exact same specification as in Column [7], clustering the standard errors based on the NUTS1 regional level instead of the NUTS3 level that we use in all other specifications. Clustering at the NUTS1 level is an important test for the assumption of independent and identically distributed observations. If the population of one city were affected by changes in the population of another neighbouring city, this assumption might not hold. The results presented in Column [10] confirm that our results are robust to this concern. While the first-stage F-statistic is rather lower, it is still above the Stock and Yogo (2005) 15 percent maximal IV size and the standard errors of the main highway and railway ray coefficients are hardly unchanged.

In Column [11], we address another important endogeneity concern, which is caused by including the logarithm of the LUZ as an independent variable on the right-hand side of the estimated equation. Obviously, LUZ population is partly composed by the CC population, giving rise to endogeneity concerns. In Column [11], we use the difference of the logarithm of CC and suburban population as the

[26]We do not comment on these results though as they are similar with those in Table 2.6.

dependent variable and the main results hold[27]. The change in the value of the estimated highway ray coeffecient reflects the different dependent variable, which could be interpreted in a similar way as a measure of relative suburbanization.

Table 2.6: Robustness checks

Dependent variable:	ln(POP^{CC})	Δln(POP^{CC-SUB})	ln(POP^{CC})	ln(POP^{CC})	ln($POP^{CC}_{c.\ mun.}$)	ln($POP^{CC}_{50\%POP}$)
	[10]	[11]	[12]	[13]	[14]	[15]
Highway rays	-0.094^{a}	-0.219^{a}	-0.095^{a}	-0.091^{a}	-0.096^{a}	-0.077^{b}
	(0.019)	(0.039)	(0.019)	(0.022)	(0.022)	(0.038)
Railroad rays	-0.003	0.030	-0.003	0.033^{b}	0.015	0.033
	(0.020)	(0.037)	(0.016)	(0.016)	(0.029)	(0.023)
ln(LUZ population)	✓	✓	✓	✓	✓	
NUTS1 clustering	✓					
Smoothed rays			✓			
Algorithm rays				✓		✓
First-Stage F-statistic	5.5	9.2	16.8	15.6	9.3	4.8
Observations	3,474	3,474	3,474	3,474	3,474	3,474

Notes: The estimates presented in table 2.6 include 579 cities in 6 decades (1961-2011). All regressions include LUZ fixed effects and NUTS1-specific time fixed effects. Our historical instruments are smoothed; i.e. they are time varying and they are computed by multiplying the number of historical rays/length by the fraction of the highway/railway mileage in each country completed at each decade. The smoothed 1810 postal route rays and the smoothed 1870 railroad rays instrument for highway and railroad rays, respectively. The Stock & Yogo (2004) 10 percent critical value is 7 for two instrumented variables. Robust standard errors are clustered by NUTS3 regions (except stated otherwise) and are in parenthesis. ^{a}, ^{b} and ^{c} indicates significant at 1, 5, and 10 percent level, respectively.

Column [12] includes the 'smoothed rays' measure of highways and railways. Here, they are computed by multiplying the number of 2011 highway/railway rays by the fraction of the highway/railway mileage in each LUZ completed in each decade. The fractional values of the rays measure allows even small suburbanization effects to show up in the coefficient. This could be the case if, for example, it takes twenty years for residential location patterns to fully respond to changes in highway infrastructure. The results of our preferred specification using the smoothed rays remain unchanged.

Column [13] uses the number of rays based on the algorithm count that we described in Section 2.2.1. Using this alternative definition of highway rays, we confirm the highway coefficient in our preferred specification while we also find a statistically significant effect of railway rays. We consider that this latter effect is caused because our algorithm overcounts the number of railroad rays, as we discussed in Section 2.2.1.

As discussed in Section 2.2.1, Eurostat defines LUZs based on a harmonized

[27]We have also used the lagged LUZ population to control for simultaneous causality bias and the main results hold as well.

methodology using commuting criteria, which makes LUZs the most appropriate spatial unit for the analysis of suburbanization in Europe. However, the definitions of the CC and the suburbs are modern definitions. Therefore, the spatial units of our analysis may have been defined based on the actual suburbanization patterns observed in each city. We address this issue by constructing alternative CC boundaries, using the municipality population data. Column [14] defines the CC as the central municipality in each city, which has the same name as the city.

Very few of the central municipalities have been subject to municipality mergers since they have historically comprised a significant part of the city population. Therefore, using the 'one-municipality' definition can be regarded as a 1961 'constant geography' CC definition. The problem with this definition is related to the discussion in Section 2.2.1 about the limitations of the algorithm ray counts. However, in the case of the one-municipality definition, the problem is not only that the highways are undercounted but also that the 1810 postal routes are considerably undercounted. Therefore, using the algorithm count of rays, we have a very weak instrument, which does not permit any robust estimation[28]. On the other hand, as can be seen in Column [15], where we include the measure of rays based on Eurostat's CC (counted manually or using the algorithm), the results follow our main findings. These results are in line with our main results even when we include the one-municipality measures of suburban ramps and stations and the corresponding smoothed instruments.

Column [16] uses an alternative definition of the CC based on the municipalities that compose the 50 percent of the 1961 LUZ population[29]. Since the 50 percent of the 1961 LUZ population is in general a bigger area than Eurostat's CC, we can use the algorithm count for this specification[30]. The highway ray coefficient of Column [16] is slightly lower than the estimated coefficient in our preferred specification. This change was expected because we limit the suburban area and increase the CC. All these specifications confirm not only our preferred specification [7] but they are also in line with the rest of specifications in Table 2.5.

[28]Nonetheless, the highway ray coefficient has roughly the same value.

[29]We have defined this core starting from the central municipality of the city and adjoining one-by-one the closest municipalities until we reach the 50 percent of the 1961 LUZ population. Using a higher population threshold is not very meaningful because the CC area becomes too big to measure any measure of highway and railway penetration. On the other hand, a CC definition based on the municipalities that comprise up to 25 percent of each LUZ 1961 population coincides with the one-municipality definition.

[30]Results hold fot the Eurostat's CC rays as well

2.4 Heterogeneous effects

2.4.1 Suburbanization by time period

According to urban economic theory, households respond to the increase in accessibility to the Central Business District (CBD) by relocating from the central city to the suburbs. However, the reaction of households to improvements in transport infrastructure appears to have varied considerably during our 50-year study period. There are a number of circumstances that point out to this variation. In Table 2.1, Section 2.2.1, we saw that the LUZ population growth was highest in the decade 1961-1971 and almost twice that of the second highest period of growth which occurred between 1971 and 1981. In addition, Table 2.2 in Section 2.2.1 indicates that during this first decade, small cities experienced intense urbanization while their bigger counterparts underwent extensive suburbanization. However, this pattern became more balanced in terms of suburbanization across all city sizes towards 2011.

Table 2.7: Time periods

Dependent variable:	ln(Central city population)						
	[1]	[2]	[3]	[4]	[5]	[6]	[7]
Panel A	1961–1971–1981						
Highway rays	-0.081^{a}		-0.123^{a}			-0.080^{a}	
	(0.014)		(0.035)			(0.014)	
ln(suburban ramps)		-0.030^{a}	0.053^{c}				-0.029^{a}
		(0.009)	(0.029)				(0.009)
Railroad rays*				-4.734			
				(3.797)			
ln(suburban stations)					-0.155^{b}	-0.145^{c}	-0.149^{b}
					(0.067)	(0.077)	(0.070)
First-Stage F-statistic	56.6	202.7	11.2	1.6	72.1	38.6	36
Observations	1,737	1,737	1,737	1,737	1,737	1,737	1,737
Panel B	1991–2001–2011						
Highway rays	-0.042^{b}		-0.028^{a}			-0.043^{b}	
	(0.021)		(0.009)			(0.022)	
ln(suburban ramps)		-0.039	0.010				
		(0.024)	(0.011)				
Railroad rays*				-0.012			
				(0.008)			
ln(suburban stations)					-0.008^{c}	-0.007^{c}	
					(0.004)	(0.004)	
First-Stage F-statistic	9.7	10.6	11.7	23.8	171.6	4.8	
Observations	1,737	1,737	1,737	1,737	1,737	1,737	

Notes: *The railway rays coefficient for the period 1961–1981 is obtained using the smoothed railway rays' measure. The selection of the specifications included is explained in Section 2.3.3. All regressions include the log of LUZ population, LUZ fixed effects and NUTS1-specific year fixed effects. Our historical instruments are smoothed; i.e. they are time varying and they are computed by multiplying the number of historical rays/length by the fraction of the highway/railway mileage in each country completed at each decade. The Stock & Yogo (2004) 10 percent critical values are 16.4 and 7 for one and two instrumented variables, respectively. Robust standard errors are clustered by NUTS3 regions and are in parenthesis. ^{a}, ^{b} and ^{c} indicates significant at 1, 5, and 10 percent level, respectively.

In this section, we first split our period of study in two in order to test whether the effect of transportation infrastructure on suburbanization differed between these subperiods. Table 2.7 shows the results when we split the study period (1961-2011) into two subperiods: 1961-1981 (Panel A) and 1991-2011[31] (Panel B). There is a statistically significant difference between the highway coefficient in the period 1961-1981 (Column [6] in Panel A) and the lower coefficient in the period 1991-2011 (Column [6] in Panel B). This finding could imply that our average results were mainly driven by the first subperiod or by the cities in which highways were constructed during the 1960s and 1970s. This is the main reason why in Table 2.8, we use two subsamples of cities based on the existence of one or more highways by 1981 and we also split the whole 50-year period in the two subperiods used in Table 2.7. Another interesting result from Table 2.7, Panel A, is that suburban railway stations are highly statistically significant with a high value during the first sub-period, while they are only marginally significant with a very low coefficient in the second sub-period. Both highway and railway coefficients indicate that the effect of transport infrastructure on suburbanization was significantly higher (at the 10 percent level) in the period 1961-1981.

Panel A of Table 2.8 shows that for the cities with highways in 1981, the highway effect on suburbanization in the whole period of study is roughly the same as in our preferred specification (Column [7] in Table 2.5). Panel B shows the same effect for each of the two subperiods. As can be seen, there is a highly significant effect of highways on suburbanization during the first period but no effect in the period 1991-2011. This first result suggests that the early highways that were opened before 1981 fostered the suburbanization of the cities in which they were constructed during the period 1961-1981. In contrast, the latter result suggests that in the cities with some highway endowments by 1981, the additional highways built after 1981 did not cause any further suburbanization during the period 1991-2011[32]. This finding indicates that the effect of highway developement on suburbanization is decreasing in the number of rays[33].

Panel C in Table 2.8 includes only those cities that had no highways up until 1981. We created this subsample in order to test whether our average results were solely attributable to those cities in which highways were constructed early. The results for the whole period suggest an individual highway coefficient that is higher than in our average results and statistically significant at the 5 percent level. This finding seems

[31]The results are similar when other subperiods were considered (1961-1991 and 1991-2011, as well as 1961-1981 and 1981-2011). We use the current periods in order to have two 20-year periods that facilitates their comparison.

[32]In cities with highways built by 1981, many new highways were also constructed after 1981 (47 percent increase in the total number of highways in the period 1981-2011 in these cities).

[33]We cannot consistently estimate a specification with a quadratic term using IV regression.

to be the result of the other part of Table 2.2.2, Section 2.2.1, where the smaller cities were the ones that greatly urbanized during this period. However, the strength of our instrument does not allow us to interpret this result further. In addition, the results for the 1991-2011 subperiod present a noticeably lower highway coefficient which further supports this last claim. Nonetheless, Panel C suggests that highways also caused suburbanization in the cities with highways constructed after 1981.

Table 2.8: Highway construction period and subperiods

Dependent variable:	ln(Central city population)								
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]
Panel A: 327 LUZs with highways prior to 1961 or built between 1961 and 1981									
	1961–2011								
Highway rays	-0.100^a		-0.112^a			-0.100^a			
	(0.027)		(0.032)			(0.027)			
ln(suburban ramps)		-0.041^c	0.071^c				-0.040^c		
		(0.021)	(0.038)				(0.021)		
Railroad rays*				-0.061^b		-0.004	-0.064^b		
				(0.025)		(0.015)	(0.026)		
ln(suburban stations)					-0.008				
					(0.015)				
First-Stage F-statistic	38.5	109.2	18.9	22.8	85.9	12.1	18.4		
Observations	1,962	1,962	1,962	1,962	1,962	1,962	1,962		
Panel B: 327 LUZs with highways prior to 1961 or built between 1961 and 1981									
	1961–1981				1991–2011				
Highway rays	-0.084^a				-0.004				
	(0.020)				(0.027)				
ln(suburban ramps)		-0.016				-0.008			
		(0.012)				(0.063)			
Railroad rays*			5.891				-0.005		
			(8.027)				(0.007)		
ln(suburban stations)				-0.006				-0.004	
				(0.050)				(0.006)	
First-Stage F-statistic	26	91.4	0.5	8219	4.6	1.3	23.2	77.2	
Observations	981	981	981	981	981	981	981	981	
Panel C: 252 Other LUZs (no highways until 1991)									
	1961–2011					1991–2011			
Highway rays	-0.143^b		-0.211^c			-0.066^b			
	(0.063)		(0.109)			(0.029)			
ln(suburban ramps)		-0.053^b	0.185				-0.051		
		(0.022)	(0.123)				(0.033)		
Railroad rays				-0.086				-0.036	
				(0.070)				(0.033)	
ln(suburban stations)					0.022				-0.006
					(0.016)				(0.007)
First-Stage F-statistic	8.8	33.7	2.2	1.9	55.7	4.7	7	1.6	64.8
Observations	1,512	1,512	1,512	1,512	1,512	756	756	756	756

Notes: *The railway rays coefficient for the period 1961–1981 is obtained using the smoothed railway rays' measure. The selection of the specifications included is explained in Section 2.3.3. All regressions include the log of LUZ population, LUZ fixed effects and NUTS1-specific year fixed effects. Our historical instruments are smoothed; i.e. they are time varying and they are computed by multiplying the number of historical rays/length by the fraction of the highway/railway mileage in each country completed at each decade. The Stock & Yogo (2004) 10 percent critical values are 16.4 and 7 for one and two instrumented variables, respectively. Robust standard errors are clustered by NUTS3 regions and are in parenthesis. ^{a}, ^{b} and ^{c} indicates significant at 1, 5, and 10 percent level, respectively.

The results in this section suggest that the average results presented in Section 2.3.3 hold in general for all the cities and for the whole period of our dataset. In particular, considering the whole sample of cities, we find a reduced but significant effect in the later period. In addition, we find that highways caused suburbanization in the cities in which highways were constructed only after 1981. However, in all these results, the estimated effect of transport infrastructure on suburbanization declined over time. Finally, the effect of railways on suburbanization seems to follow the same pattern as the effect of highways over time.

2.4.2 Suburbanization in big cities

The descriptive statistics of suburbanization in Table 2.2, Section 2.2.2, indicate that the process of suburbanization in Europe differed for cities of different population sizes. In addition, in Map 2.1, we observe a mixed pattern of urbanization/suburbanization in Europe's cities. Following these statistics, we investigate the effect of highways and railways on suburbanization when we split our sample based on city size and city density.

Panel A in Table 2.9 presents the results when we split the total sample of cities based on the median LUZ population in 1961 (177,158 inhabitants). Our preferred specification for the big cities (Column [6]) shows that only highways are statistically significant when highway and railway rays are considered jointly. In contrast, for small cities, none of the transport infrastructure measures is statistically significant. This result makes intuitive sense since housing needs and commuting are more salient in big cities. However, the lower highway ray coefficient for the big cities indicates that the impact of highway congestion in the big cities limits the effect of highway development on suburbanization (Christidis and Ibáñez Rivas, 2012). Another explanation could be the provision of amenities in the centres of big cities. We discuss more in detail about the role of historical and other amenities in Section 2.4.3.

Another important aspect of the urban form, especially between cities in European and the US, is urban population density. In order to control for the differences between densely and less densely populated cities, in Panel B of Table 2.9, we split our sample according to the median LUZ population density in 1961 (178 inhabitants/km^2). The results suggest that the effect of highways on suburbanization does not differ significantly between more and less dense cities. However, in the case of dense cities, railroad rays are also statistically significant, albeit the railway coefficient is marginally statistically significant.

Table 2.9: City size and density

Dependent variable:	ln(Central city population)											
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]	[10]	[11]	[12]
Panel A: Size by population												
	290 Big LUZs (1961 pop≥177,158 inhab.)						289 Small LUZs (1961 pop<177,158 inhab.)					
Highway rays	-0.052^{a}		-0.056^{a}			-0.051^{a}	-0.067					
	(0.017)		(0.022)			(0.020)	(0.047)					
ln(suburban ramps)		-0.022^{c}	0.035					-0.011				
		(0.013)	(0.027)					(0.020)				
Railroad rays				-0.036^{a}		-0.003			-0.075			
				(0.012)		(0.014)			(0.056)			
ln(suburban stations)					0.016					0.011		
					(0.011)					(0.017)		
First-Stage F-statistic	22.1	157.6	8.6	18.13	132.7	5.1	11.9	101.1	5	73.5		
Observations	1,740	1,740	1,740	1,740	1,740	1,740	1,734	1,734	1,734	1,734		
Panel B: 1961 Density												
	289 Dense LUZs (1961 LUZ den ≥178 inh/km^2)						290 Sparse LUZs (1961 LUZ den<178 inh/km^2)					
Highway rays	-0.081^{a}		-0.088^{a}			-0.077^{a}	-0.090^{a}		-0.106^{a}			-0.088^{a}
	(0.023)		(0.030)			(0.023)	(0.027)		(0.036)			(0.027)
ln(suburban ramps)		-0.062^{a}	0.030					-0.046^{a}	0.057			
		(0.017)	(0.038)					(0.014)	(0.036)			
Railroad rays				-0.073^{a}		-0.028^{c}				-0.124		
				(0.027)		(0.016)				(0.088)		
ln(suburban stations)					-0.011						0.031^{b}	0.020
					(0.013)						(0.014)	(0.016)
First-Stage F-statistic	51.3	102.2	15.1	19	107.6	12.3	20.8	111.6	6.8	1.9	62.9	10.1
Observations	1,734	1,734	1,734	1,734	1,734	1,734	1,740	1,740	1,740	1,740	1,740	1,740

Notes: The selection of the specifications included is explained in Section 2.3.3. All regressions include the log of LUZ population, LUZ fixed effectsand NUTS1-specific year fixed effects. Our historical instruments are smoothed; i.e. they are time varying and they are computed by multiplying the number of historical rays/length by the fraction of the highway/railway mileage in each country completed at each decade. The Stock & Yogo (2004) 10 percent critical values are 16.4 and 7 for one and two instrumented variables, respectively. Robust standard errors are clustered by NUTS3 regions and are in parenthesis. ^{a}, ^{b} and ^{c} indicates significant at 1, 5, and 10 percent level, respectively.

2.4.3 Cities with history

Table 2.10 presents the results when separating the sample according to the cities that were considered major urban centres during different historical time periods from those that were not. Here, we find statistically significant differences between the highway rays coefficients for cities that were major Roman cities, major Medieval cities and major Pre-Industrial Revolution cities. Historical urban amenities, which are usually embedded in the central cities of historical European cities, offer a plausible explanation for these differences.

Table 2.10: City history

Dependent variable:	ln(Central city population)												
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]	[10]	[11]	[12]	[13]
Panel A: The Roman Empire													
	225 Roman cities						354 Non-Roman cities						
Highway rays	-0.052^a		-0.051^b			-0.050^a	-0.140^a		-0.156^a			-0.165^a	
	(0.015)		(0.023)			(0.016)	(0.040)		(0.047)			(0.064)	
ln(suburban ramps)		-0.065^b	-0.001					-0.045^a	0.092^b				-0.046^a
		(0.027)	(0.045)					(0.013)	(0.042)				(0.014)
Railroad rays				-0.076^b		-0.023				-0.045^b		0.059	-0.052^b
				(0.030)		(0.020)				(0.022)		(0.047)	(0.023)
ln(suburban stations)					-0.005						0.016		
					(0.023)						(0.011)		
First-Stage F-statistic	62.6	76.5	12.1	12.5	101.4	6.3	14.7	144.4	6.9	9.6	94	2.1	4.6
Observations	1,350	1,350	1,350	1,350	1,350	1,350	2,124	2,124	2,124	2,124	2,124	2,124	2,124
Panel B: The Middle Ages													
	296 Major medieval cities						283 Other cities						
Highway rays	-0.080^a		-0.084^a			-0.078^a	-0.126^c				-0.126		
	(0.016)		(0.021)			(0.021)	(0.070)				(0.079)		
ln(suburban ramps)		-0.061^a	0.017					-0.012					
		(0.014)	(0.027)					(0.019)					
Railroad rays				-0.092^a		-0.009			-0.058^b		0.000		
				(0.035)		(0.030)			(0.028)		(0.038)		
ln(suburban stations)					0.006					-0.009			
					(0.016)					(0.016)			
First-Stage F-statistic	46.8	164.9	21.2	8	155.6	2.5	4	64.3	7.4	79.5	1.4		
Observations	1,776	1,776	1,776	1,776	1,776	1,776	1,698	1,698	1,698	1,698	1,698		
Panel C: Pre-Industrial Revolution													
	357 Major cities in 1700–1750 ($\geq$ 25,000 inhab.)						222 Other cities						
Highway rays	-0.070^a		-0.073^a			-0.020	-0.120^a		-0.150^a			-0.119^a	
	(0.016)		(0.020)			(0.046)	(0.033)		(0.046)			(0.032)	
ln(suburban ramps)		-0.064^a	0.016					-0.033^b	0.093^b				-0.034^b
		(0.016)	(0.028)					(0.015)	(0.044)				(0.017)
Railroad rays				-0.130^a		-0.107				-0.044^c		0.011	-0.047^c
				(0.050)		(0.081)				(0.023)		(0.014)	(0.025)
ln(suburban stations)					0.003						-0.013		
					(0.013)						(0.018)		
First-Stage F-statistic	49	121.5	18.4	6.1	99.7	1	16.9	106.2	5.6	14.1	67.7	7.9	7.6
Observations	2,142	2,142	2,142	2,142	2,142	2,142	1,332	1,332	1,332	1,332	1,332	1,332	1,332
Panel D: Post-Industrial Revolution													
	291 Major cities in 1850 ($\geq$ 25,000 inhab.)						288 Other cities						
Highway rays	-0.075^a		-0.084^a			-0.075^a	-0.062^c				-0.057^c		
	(0.018)		(0.024)			(0.018)	(0.032)				(0.033)		
ln(suburban ramps)		-0.052^a	0.043					-0.024					
		(0.014)	(0.034)					(0.018)					
Railroad rays				-0.071^b		-0.001			-0.060				
				(0.029)		(0.018)			(0.038)				
ln(suburban stations)					-0.005					0.032^c	0.026		
					(0.016)					(0.017)	(0.019)		
First-Stage F-statistic	33	99	11.1	9.3	72.4	4.9	14.2	108.8	6.7	77.7	6.8		
Observations	1,746	1,746	1,746	1,746	1,746	1,746	1,728	1,728	1,728	1,728	1,728		

Notes: The selection of the specifications included is explained in Section 2.3.3. All regressions include the log of LUZ population, LUZ fixed effectsand NUTS1-specific year fixed effects. Our historical instruments are smoothed; i.e. they are time varying and they are computed by multiplying the number of historical rays/length by the fraction of the highway/railway mileage in each country completed at each decade. The Stock & Yogo (2004) 10 percent critical values are 16.4 and 7 for one and two instrumented variables, respectively. Robust standard errors are clustered by NUTS3 regions and are in parenthesis. ^{a}, ^{b} and ^{c} indicates significant at 1, 5, and 10 percent level, respectively.

Brueckner et al. (1999) define historical amenities as being "generated by monuments, buildings, parks, and other urban infrastructure from past eras that are aesthetically pleasing to current residents of the city". They also suggest that there is a positive correlation between historical and modern amenities. In the same line, Koster et al. (2016) suggest that "historic amenities in historic districts are generated by listed buildings, monuments, parks and the urban infrastructure from past times, but it is especially the combination of these features that generate amenities, which one typically refers to as an ensemble effect". In Table 2.14, Section 2.6.3, we present some results for cities with coast and for cities with a navigable river that they are in the same line. Therefore, urban amenities seem to explain the fact that transport infrastructure displaced less CC population in the Roman and the Medieval cities.

On the other hand, we hardly find any difference between the highway coefficients of the post-Industrial Revolution cities and the rest of the sample. Industrialization in European cities frequently occurred in a disconnected fashion from any previous urban development, hence by-passing a city's historic role as a convenient market-place, a safe bastion or a religious or political centre (Hohenberg and Lees, 2009). Some of these cities, such as London, Cologne or Amsterdam, served important functions, but many others had previously been merely villages or small towns (Plöger, 2013). The emergence of major cities during the Industrial Revolution in places with 'no history' might add to the previous explanation concerning historical urban amenities. This observation could also explain why highways in Post-Industrial Revolution cities promoted even more suburbanization than in the rest of the cities.

2.4.4 Common European grounds

In this section, we divide the cities according to the European region in which they are located. Table 2.11 presents the results when we separate our sample of cities on the basis of three greater geographical areas that shared common historical and development paths (namely, Central-North, Eastern and Mediterranean countries). For this reason, in the cases of France and Germany, we have divided the national territories of each country in two: Southern France ('*le Midi*') and the rest of the France, and East and West Germany (based on its political division)[34]. We then separate the sample according to whether the NUTS1 region in which each city is

[34]We also used other groups that included the whole of France in the Mediterranean or in the Central-Northern groups and the whole of Germany in the Central-Northern group or even in the Eastern group. The results remained largely similar.

located was characterised as Objective 1 region in 1995 or in 2000[35]. This also serves as a division between poorer and wealthier regions.

The effect of highway rays on the suburbanization of the Central-Northern European cities (Panel A) is similar to our average results. However, railway rays are also statistically significant and the estimated coefficient suggests that an additional railway ray displaced about 3.2 percent of their CC population. Central-Northern European cities are characterized in general by high economic performance, high migration inflows and well-organized urban planning systems that seek to limit urban sprawl and protect green areas around the city fringe (Couch et al., 2008). However, these results suggest that transport infrastructure affected suburbanization equally as in the rest of Europe and in the US.

The results for Eastern European cities in Columns [1]-[5] in Panel B seem to be in line with the findings of Bertaud (1999, 2006) and Redfearn (2006). Following the transition, these ex-Soviet regions had poor and very limited infrastructure that could not support the high residential densities of their city centres. In addition, the expansion of office and retail space in their city centres at the expense of residential areas, together with increased motorization and the construction of new highways and railways, fostered greater rates of suburbanization in these cities than in the cities of the rest of Europe.

The magnitude of the coefficient of highway rays for the Mediterranean cities in Columns [6]-[12] in Panel B, Table 2.11, is in line with the estimates of Garcia-López et al. (2015) for the effects of highways for Spain and with our average results. In addition, it is clear that the effect of highways on suburbanization can be attributed to highway rays, rather than the number of suburban ramps. On the other hand, due to the low instrument strength of Columns [4] and [6], it is not clear whether railways drove suburbanization in these cities.

Finally, in Panel C, Table 2.11, we split the group of NUTS1 regions between Objective 1 regions and the rest. Objective 1 regions are those whose regional GDP per capita was below 75 percent of the EU average. This grouping is meaningful because an enormous amount of the EU Regional Funds were allocated to Objective 1 (considerably less to Objective 2) regions for the construction of transport infrastructure (mainly highways). The rest of the regions received almost no funds for transport infrastructure investments from the Regional and Cohesion Funds. We find virtually no difference between the highway coefficient of the cities of Objective 1 regions and the cities of the other regions. However, for these latter cities, railways also seem to have caused suburbanization.

[35]For reasons of data availability, we use regional GDP per capita figures from 1995 for the EU15 states and from 2000 for the rest of the countries.

Table 2.11: Geographical and EU regions

Dependent variable:	ln(Central city population)										
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]	[10]	[11]
Panel A: Central-North European countries' cities											
	239 Central-North LUZs										
Highway rays	-0.088^a		-0.102^a			-0.109^a					
	(0.020)		(0.026)			(0.032)					
ln(suburban ramps)		-0.035^a	0.049^b				-0.034^a				
		(0.010)	(0.025)				(0.010)				
Railroad rays				-0.024^a		0.032^c	-0.027^a				
				(0.007)		(0.020)	(0.008)				
ln(suburban stations)					-0.005						
					(0.010)						
First-Stage F-statistic	26.9	134.5	12.4	22.8	82.6	4.6	12.3				
Observations	1,434	1,434	1,434	1,434	1,434	1,434	1,434				
Panel B: Eastern European and Mediterranean countries' cities											
	147 Eastern LUZs					193 Mediterranean LUZs					
Highway rays	-0.149^b				-0.137^b	-0.082^a		-0.079^b			-0.052
	(0.070)				(0.066)	(0.026)		(0.033)			(0.034)
ln(suburban ramps)		-0.011					-0.123^a	-0.022			
		(0.026)					(0.033)	(0.058)			
Railroad rays			-0.750						-0.198^b		-0.142
			(3.266)						(0.096)		(0.087)
ln(suburban stations)				0.038^b	0.042^a					-0.018	
				(0.015)	(0.014)					(0.028)	
First-Stage F-statistic	4.7	38.9	0.0	49.2	2.4	45.0	85.9	10.2	7.6	52.3	2.2
Observations	882	882	882	882	882	1,158	1,158	1,158	1,158	1,158	1,158
Panel C: EU regional policy (Objective 1)											
	242 LUZs in 1996–2011 Objective 1					337 Other LUZs					
Highway rays	-0.087^b		-0.101^c			-0.074^a		-0.083^a			-0.083^a
	(0.037)		(0.053)			(0.015)		(0.019)			(0.019)
ln(suburban ramps)		-0.059^b	0.055				-0.037^a	0.052^c			
		(0.026)	(0.068)				(0.013)	(0.026)			
Railroad rays				-0.154					-0.034^a		0.024^c
				(0.120)					(0.012)		(0.015)
ln(suburban stations)					0.020					0.006	
					(0.017)					(0.011)	
First-Stage F-statistic	14.6	97.9	3.3	1.8	56.3	43.5	151.1	19.2	22.5	151	8
Observations	1,476	1,476	1,476	1,476	1,476	1,632	1,632	1,632	1,632	1,632	1,632

Notes: The Mediterranean regions include Bulgaria, Croatia, Cyprus, the South of France, Greece, Italy, Malta, Portugal and Spain. The East European countries regions include Austria, Czech Republic, Estonia, Finland, Eastern Germany, Hungary, Latvia, Poland, Romania, and Slovakia. Finally, the Central-North regions include Belgium, Denmark, France (except for the South), Western Germany, Ireland, Iceland, Luxembourg, the Netherlands, Norway, Sweden, Switzerland and the United Kingdom. Objective 1 cities are those whose NUTS2 regional GDP per capita was below the 75 percent of the EU average in 1995 or in 2000 (if data for 1995 are not available). The selection of the specifications included is explained in Section 2.3.3. All regressions include the log of LUZ population, LUZ fixed effects and NUTS1-specific year fixed effects. Our historical instruments are smoothed; i.e. they are time varying and they are computed by multiplying the number of historical rays/length by the fraction of the highway/railway mileage in each country completed at each decade. The Stock & Yogo (2004) 10 percent critical values are 16.4 and 7 for one and two instrumented variables, respectively. Robust standard errors are clustered by NUTS3 regions and are in parenthesis. ^{a}, ^{b} and ^{c} indicates significant at 1, 5, and 10 percent level, respectively.

2.5 Conclusions

During the second half of the 20th and the beginning of the 21st centuries, European national governments and the EU have allocated a vast amount of resources to highway construction. However, in contrast to the US, railways are also very popular as a way of commuting and thus, highways and railways should be considered jointly when analysing the effect of transport infrastructure in Europe's cities. In this paper, we estimate the joint effect of highways and railways on the suburbanization of 579 cities located in 29 countries for the period 1961-2011. To the best of our knowledge, this is the first paper to estimate this effect for such a unique sample of cities and countries. In addition, this is one of very few studies to consider the whole of Europe and in so doing, it offers valuable insights into the heterogeneity of European cities and into the different urban processes operating in Europe and in the US.

Our estimates suggest that an additional highway ray displaced, on average, approximately 9 percent of the central city population in European cities during the period 1961-2011. However, we find no effect of railways on suburbanization when the two modes are considered together. We further exploit our rich dataset to validate our main findings and to obtain heterogeneous estimates. We find evidence that the effect of transport infrastructure on suburbanization was significantly weaker in the period 1991-2011 than in the period 1961-1981. Additionally, we confirm that the average suburbanization effect is driven both by those cities that had highways since the early years in our sample and by cities that built highways at the end of the 20th century too. Nevertheless, we find that the effect of highways on suburbanization has decayed over time in the case of European cities. This is an important and novel result, which in part defends EU highway funding in recent decades. This position is further supported by the estimated effects of highways on the suburbanization of cities that received most of the EU Regional and Cohesion Funds, when compared with the rest of the cities.

In line with the literature that highlights the importance of history for Europe's system of cities, we test whether the effect of transportation infrastructure on suburbanization varies when cities that prospered during different historical periods are considered. Our findings suggest that the effect of highways on suburbanization varies considerably in line with certain characteristics of historical cities. Specifically, we find significant variation in the estimated effect for highways in cities that were major centres during the Roman and the Medieval eras. Moreover, we find that these differences decline as we gradually consider cities with 'less history'. These results appear to be related to the historical and other urban amenities embedded in the city centres of many historical European cities that make these cities more resilient to suburbanization. This finding has major implications for urban economic theory and points to marked differences between European and US cities.

We further explore the heterogeneous patterns of suburbanization detected across Europe, by separately analysing the bigger and the smaller cities, as well as the more densely and the less densely populated cities. In the latter case, no significant differences were found in the estimated effects for highways; however, for big cities, we found that highways had a significant effect on suburbanization while for the smaller cities we found no statistically significant effect. This result makes intuitive sense since housing needs and commuting are more salient in big cities.

We also find interesting differences between cities located in different geographical regions of Europe. Specifically, cities in the Eastern European regions were more markedly affected by highways than an average European city. Additionally, in the cities of Central-Northern Europe, railways were also important drivers of suburbanization. Finally, we find that highways caused significantly less suburbanization in coastal cities and in the cities with navigable rivers. This result seems to provide further support for the importance of amenities – not only historical, but natural – in central cities. All these findings are especially relevant and complement some seminal papers published in the related literature. Finally, the outcomes of this paper provide valuable insights for the European regional and transport policies.

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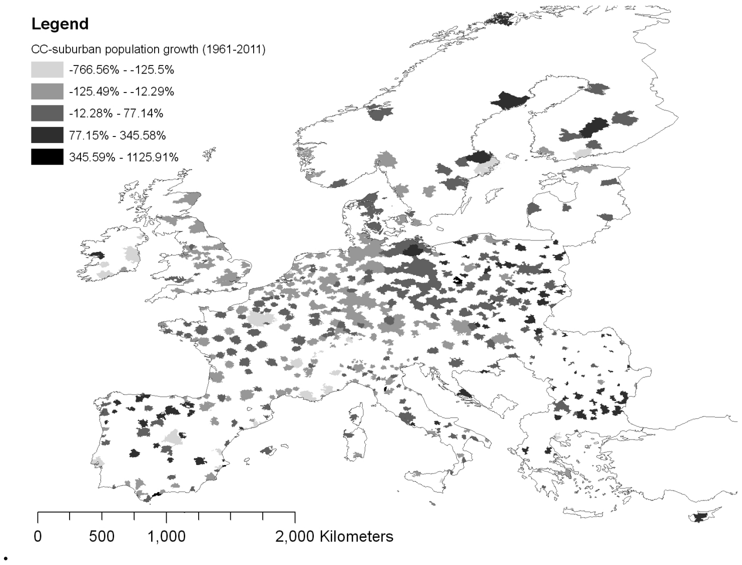
2.6 Appendix

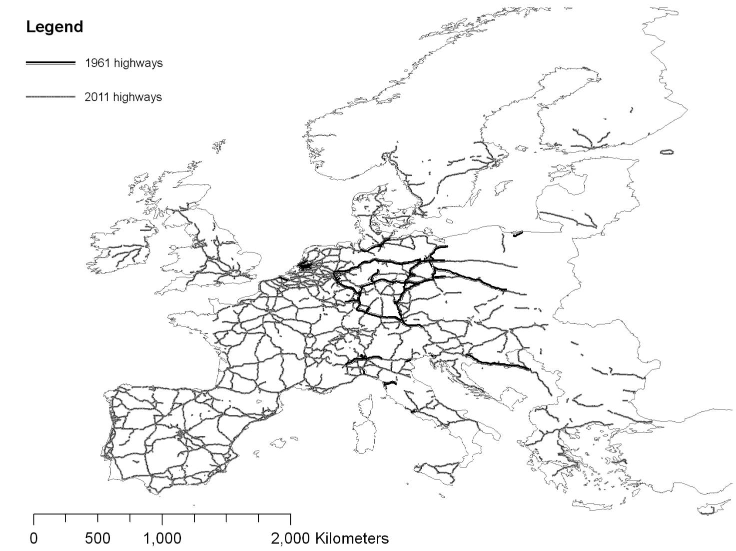
2.6.1 Maps

Figure 2.1: Average relative (sub)urbanization in European cities (1961-2011).



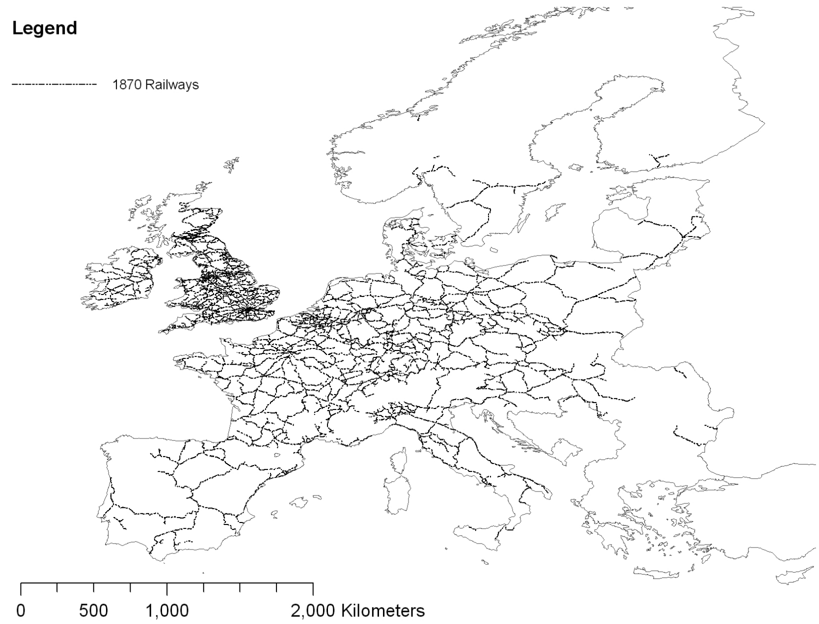
Source: Authors' own calculations based on the DG-REGIO census municipal population data.

Figure 2.2: Evolution of highways (1961-2011)



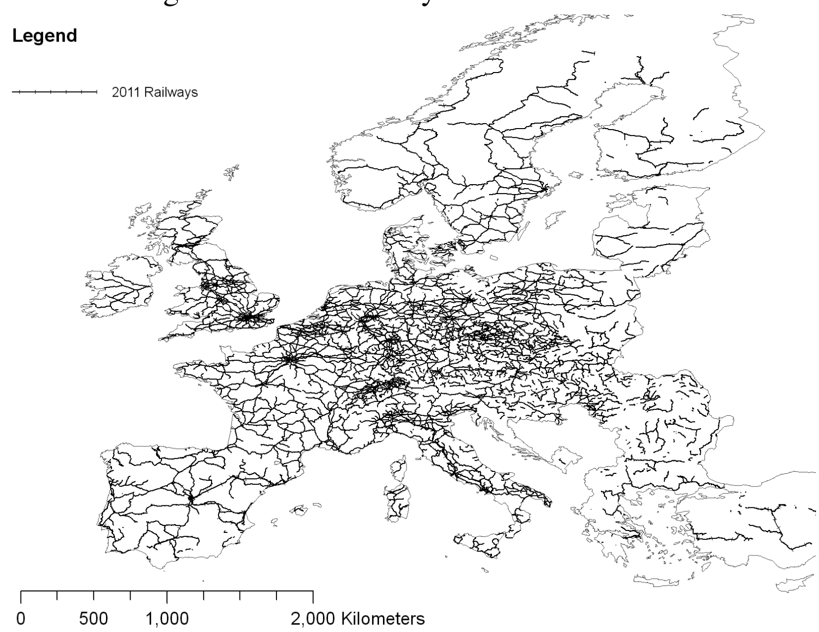
Source: Authors' own calculations based on the RRG database.

Figure 2.3: The railway network in 1870.



Source: Authors' own calculations based on the map from the Historical GIS for European Integration Studies.

Figure 2.4: The railway network in 2011.



Source: Authors' own calculations based on the RRG database.

2.6.2 Additional robustness results

Table 2.12 shows the first-stage of a long-difference specification that includes a number of historical and geographical control variables[36].

Table 2.12: Long-difference first stage results

	2011 variables			
Dependent variable:	Highw. rays	ln(sub. ramps)	Railw. rays	ln(sub. stations)
	OLS [1]	OLS [2]	OLS [3]	OLS [4]
1810 postal route rays	0.103^b (0.041)			
ln(1810 postal route km)		0.061^a (0.022)		
1870 railroad rays			0.542^a (0.110)	
ln(1870 railroad km)				0.078^a (0.021)
ln(1961 LUZ population)	✓	✓	✓	✓
2011-1961 Δln(LUZ pop.)	✓	✓	✓	✓
Country FE	✓	✓	✓	✓
History	✓	✓	✓	✓
Geography	✓	✓	✓	✓
R^2	0.594	0.723	0.620	0.805
Observations	579	579	579	579

Notes: In Columns [1]-[4], geography is controlled by the logarithm of the CC and the LUZ area, the mean and range of CC elevation, the mean surface ruggedness for each LUZ and the logarithm of the distance to the closest coast from the CC centroid. History is controlled by the inclusion of dummy variables for historical major cities (in 1000 and 1450) and for the logarithm of city population in 1850, for cities with universities between the 12^{th} and 15^{th} century, for cities with Roman settlements, for cities with bishoprics (in 600 and 1450), for cities with medieval monasteries and for cities with historical city centres or another landmark denominated by UNESCO. Robust standard errors are clustered by NUTS3 regions and are in parenthesis. ^{a}, ^{b} and ^{c} indicates significant at 1, 5, and 10 percent level, respectively.

By including a series of historical variables, we show that even when we explicitly control for past economic development and political influence, the historical transport variables we use as instruments are still highly statistically significant and positively related with the modern transport infrastructure[37]. A further concern for

[36] However, we dropped the 2nd-stage estimates of the long-difference specification because we consider the panel estimation to be substantially more robust than the former.

[37] Europe's biggest cities in 1000, 1450 and the logarithm of 1850 populations can be used as proxies for economic development in earlier centuries. In the past, cities were the centre of commerce and the Industrial Revolution further concentrated economic activity around major urban areas (Tabellini, 2010). Several studies have relied on city size as a measure of past economic development (De Long and Shleifer, 1993; Acemoglu et al., 2005). On the other hand, the inclusion of dummies for cities with bishoprics, medieval monasteries, Roman settlements and monasteries can be regarded as proxies for political influence in the past.

the first-stage estimation is that geographical features may have affected the location of both modern and historical transport infrastructure. The literature has reported a negative relationship between surface roughness and transport infrastructure (Ramcharan, 2009), which appears to be consistent with the road construction literature. The estimates suggest an exponential impact of terrain grade variation on the cost of building and maintaining roadways and rail lines, as well as on the time and energy required to move goods within a country and to maintain transport networks[38].

Table 2.13: Additional robustness checks

Dependent variable:	$\ln(POP^{CC})$	$\Delta\ln(POP^{CC-SUB})$	$\ln(POP^{CC})$	$\ln(POP^{CC})$	$\ln(POP^{CC}_{c.\ mun.})$	$\ln(POP^{CC}_{50\%POP})$
	[16]	[17]	[18]	[19]	[20]	[21]
Highway rays	-0.054^{b}	-0.274^{a}	-0.118^{a}	-0.117^{a}	-0.118^{a}	-0.023
	(0.023)	(0.067)	(0.032)	(0.041)	(0.033)	(0.062)
ln(suburban ramps)	-0.002	0.164^{b}	0.059^{c}	0.059	0.061	-0.087^{c}
	(0.035)	(0.078)	(0.035)	(0.043)	(0.040)	(0.049)
Railroad rays	-0.015	0.098	0.020	0.057^{c}	0.043	0.009
	(0.015)	(0.066)	(0.026)	(0.032)	(0.043)	(0.034)
ln(LUZ population)	✓	✓	✓	✓	✓	
NUTS1 clustering	✓					
Smoothed rays			✓			
Algorithm rays				✓		✓
First-Stage F-statistic	7.2	3.1	4.7	3.8	3.2	1.7
Observations	3,474	3,474	3,474	3,474	3,452	3,159

Notes: The estimates presented in table 2.13 include 579 cities in 6 decades (1961-2011). All regressions include LUZ fixed effects and NUTS1-specific time fixed effects. Our historical instruments are smoothed; i.e. they are time varying and they are computed by multiplying the number of historical rays/length by the fraction of the highway/railway mileage in each country completed at each decade. Robust standard errors are clustered by NUTS3 regions (except stated otherwise) and are in parenthesis. ^{a}, ^{b} and ^{c} indicates significant at 1, 5, and 10 percent level, respectively.

2.6.3 Additional heterogeneous results: Natural geography

A further source of heterogeneity among European cities is their natural geography. The geographical features that we consider in the heterogeneous estimates reported in Table 2.14 are contiguity to the coast and whether a city is intersected by a navigable river.

[38]See for example Aw (1981), Highway Research Board (1962) and Paterson (1987).

Table 2.14: City geography

Dependent variable:	ln(Central city population)						
	[1]	[2]	[3]	[4]	[5]	[6]	[7]
Panel A	175 Coastal LUZs						
Highway rays	-0.083^{a}		-0.082^{a}			-0.079^{a}	
	(0.024)		(0.030)			(0.028)	
ln(suburban ramps)		-0.092^{a}	-0.003				
		(0.020)	(0.036)				
Railroad rays				-0.057^{b}		-0.010	
				(0.025)		(0.024)	
ln(suburban stations)					-0.007		
					(0.017)		
First-Stage F-statistic	23.1	83.5	11.7	13.8	33.4	2.9	
Observations	1,050	1,050	1,050	1,050	1,050	1,050	
Panel B	404 Inland LUZs						
Highway rays	-0.107^{a}		-0.127^{a}			-0.109^{a}	
	(0.024)		(0.032)			(0.027)	
ln(suburban ramps)		-0.030^{a}	0.094^{b}				-0.045^{a}
		(0.011)	(0.038)				(0.017)
Railroad rays				-0.085^{a}		0.007	-0.095^{a}
				(0.032)		(0.023)	(0.035)
ln(suburban stations)					0.018		
					(0.014)		
First-Stage F-statistic	37.5	144	14.9	13.7	239.2	5	4.4
Observations	2,424	2,424	2,424	2,424	2,424	2,424	2,424
Panel C	260 Cities with navigable river						
Highway rays	-0.098^{a}		-0.111^{a}			-0.098^{a}	
	(0.023)		(0.030)			(0.036)	
ln(suburban ramps)		-0.031^{b}	0.081^{b}				-0.043^{c}
		(0.015)	(0.040)				(0.023)
Railroad rays				-0.092^{b}		0.001	-0.099^{b}
				(0.036)		(0.043)	(0.040)
ln(suburban stations)					0.007		
					(0.016)		
First-Stage F-statistic	18.6	104.8	7.5	6.946	170.8	1.7	3.3
Observations	1,560	1,560	1,560	1,560	1,560	1,560	1,560
Panel D	319 Other cities						
Highway rays	-0.121^{a}		-0.142^{a}			-0.121^{a}	
	(0.026)		(0.035)			(0.026)	
ln(suburban ramps)		-0.065^{a}	0.078^{c}				-0.072^{a}
		(0.020)	(0.043)				(0.021)
Railroad rays				-0.074^{b}		-0.005	-0.089^{b}
				(0.032)		(0.022)	(0.036)
ln(suburban stations)					0.004		
					(0.015)		
First-Stage F-statistic	29.2	109.7	12.1	11.1	62.9	6.7	5.5
Observations	1,914	1,914	1,914	1,914	1,914	1,914	1,914

Notes: The selection of the specifications included is explained in Section 2.3.3. All regressions include the log of LUZ population and NUTS1-specific time fixed effects. Our historical instruments are smoothed; i.e. they are time varying and they are computed by multiplying the number of historical rays/length by the fraction of the highway/railway mileage in each LUZ completed at each decade. The Stock & Yogo (2004) 10 percent critical values are 16.4 and 7 for one and two instrumented variables, respectively. Robust standard errors are clustered by NUTS3 regions and are in parenthesis. ^{a}, ^{b} and ^{c} indicates significant at 1, 5, and 10 percent level, respectively.

The fact that in Panel A of Table 2.14, the highway coefficient is lower for coastal cities than for the inland cities in Panel B, seems to be in line with the literature on consumer cities and reverse commuting i.e. where "commuters live in central cities and work in the suburbs" (Glaeser et al., 2001). In Panel C of Table 2.14, we present the estimation output for cities crossed by a navigable river and in Panel D, the rest of the cities. Around 60 percent of the cities with a navigable river were major Medieval and Pre-Industrial Revolution cities. Thus, the role of historical amenities (see Section 2.4.3) and the potential natural amenity of rivers could account for the lower highway coefficient in these cities[39]. Finally, the fragmentation of space caused by the presence of a river could also limit potential for suburbanization in these cities.

[39] A clean river is regarded as a positive amenity while a polluted river is regarded as a disamenity.

3 Highway congestion and air pollution in Europe's cities [§]

3.1 Introduction

Outdoor air pollution kills 3.3 million people, mostly in cities, every year (Lelieveld et al., 2015). That's more than HIV, malaria and influenza combined — yet the sparse coverage of official data suggests that many cities are not even monitored. Emissions of air pollutants in cities are, in part, driven by where and how people live (e.g. central cities vs. suburbs), work (e.g. close to work place vs. long commutes), and how they travel (e.g. private cars vs. public transportation) (Hilber and Palmer, 2014). In fact, EU's environmental legislation is working to ensure that European citizens enjoy cities with clean air and to promote better green infrastructure. Besides air pollution, another critical issue that European cities have to address is traffic congestion. The cost of road congestion in Europe is estimated to be over €110 billion a year (about 1 percent of the GDP) and it is also mainly concentrated in cities (Christidis and Rivas, 2012). INRIX and Cebr (2014) report that the cost of traffic congestion in France, Germany, UK and US between 2013 and 2030 is expected to rise by 50 percent. Based on these forecasts, the total cumulative cost of traffic congestion for these economies during these years is estimated to be about $4.4 trillion, without taking into account the cost of air pollution and CO_2 emissions. Therefore, analysing the effect of the vast investments in highway infrastructure on traffic congestion and air pollution is clearly of utter importance.

While EU Regional and Cohesion Funds have financed a considerable part of the immense highway network development in the last few decades[1], there is no integrated study that analyses the impact of highway construction on traffic congestion

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[1]During the first 15 years of its existence, the European Regional Development Fund devoted 80 percent of its funding to infrastructure projects (Vickerman, 1991) and over the period 2000-2006, about 35 percent of the Structural Funds and 50 percent of the Cohesion Fund was spent on infrastructure projects (Crescenzi and Rodríguez-Pose, 2012). During the period 2007-2013, again, approximately 35 percent of the total amount spent by the Structural and Cohesion Funds was invested in roads, mainly highways (DG-REGIO, 2016).

in the cities of the whole Europe, based on our knowledge. Nevertheless, one of the main criticisms to the expansion of an intra-metropolitan road network is that such policies may not generate any real improvements in accessibility, because of the induced demand effect or the 'fundamental law of highway congestion' (Downs, 1962, 1992) i.e. the travel speed on an expanded highway reverts to its previous level before the capacity expansion. Moreover, if the 'fundamental law' holds, the subsequent increase in car use is expected to contribute to urban air pollution in Europe's cities. Therefore, the objective of this chapter is to test the 'fundamental law of highway congestion' and estimate the effect of the increase in highway traffic on urban air pollution for 545 metropolitan areas of the EU28 countries (except for Cyprus and Malta), Norway and Switzerland in the period 1985-2005.

Traffic congestion and environmental pollution figure as two of the three most important negative externalities related to car travel, together with accidents. (Shefer and Rietveld, 1997). These externalities share the same 'external cost' nature, as the use of a vehicle generates negative side effects on the rest of the economy. However, the level of urban pollution per se does not usually reduce the level of car use, in contrast to congestion, which discourages car use directly. Thus, there is no inherent feedback mechanism. Second, the environmental damage can often be reduced with 'filter' technology and regulatory policies, i.e. changes in the technology (engine, vehicle design) that reduce the level of emissions per km or other EU policies (Air Quality Standards, Low Emission Zones etc.). "Pollution can be reduced without changing car use, which is not possible for congestion as it is a function of the number of vehicles using an infrastructure at a particular time, so either trips have to be suppressed or relocated to another infrastructure or another moment" (Proost and Van Dender, 2012).

Based on this simple logic, the first goal of this paper is to test the 'fundamental law of highway congestion' by estimating the elasticity of vehicle kilometres travelled (VKT) with respect to highway lane km. Given the feedback mechanism of traffic congestion, we need to overcome several identification issues in order to estimate the causal effect of highway construction on traffic congestion. We are able to overcome such issues by means of instrumental variables, using four different historical transportation networks in Europe as instruments, together with panel data techniques. Regarding the estimation of the subsequent effect of traffic congestion on urban air pollution[2], we avoid many identification concerns using a unique dataset, which reports the emissions of the major air pollutants that are *solely attributed to road transport*. Therefore, using highway traffic as the main variable of interest and focusing on time variation to obtain identification, we min-

[2]In this paper, we measure emissions and not explicitly air pollution. However, the two terms are used interchangeably.

imise omitted variable bias concerns. Using our unique dataset on emissions and a city fixed effects approach, we estimate the effect of highway traffic on three of the most dangerous air pollutants related to road transport, namely, nitrogen oxides (NO_X), sulphur dioxide (SO_2) and fine particulate matter (PM_{10}).

The 'fundamental law of highway congestion' has also been tested empirically extensively (for an overview, see Goodwin et al. (2004) and Noland and Lem (2002)). Most of this literature estimates short-run (five-year) and long-run elasticities of around 0.5 and 0.8 while the seminal paper of Duranton and Turner (2011), as well as Hsu and Zhang (2014) find an elasticity of VKT with respect to highway lane km of approximately one for US and Japan, respectively. A unit elasticity suggests that increasing highway supply does not reduce traffic congestion *not even partly*. However, it is not straightforward that the fundamental law should also hold for the cities of Europe. European cities seem to be rather compact compared to most American cities[3] and they are also characterised by a lower degree of car-dependency[4], the widespread use of public transportation, particularly subways[5] (Gonzalez-Navarro and Turner, 2016) and historical urban amenities in the city centres (Brueckner et al., 1999). Therefore, one might expect that the reaction of the demand side to an increase in the supply of highways might be different than in the case of US. Finally, we use the cities with toll highways to investigate the role of pricing in the 'fundamental law'. Based on the principles of congestion pricing (Walters, 1961; Vickrey, 1963), the existence of tolls could mitigate the increase in highway traffic after the development of the highways.

There has been considerable research undertaken on the impact of transportation on suburbanization or 'urban sprawl' and its effects on greenhouse gas emissions (Glaeser and Kahn, 2004, 2010; Gaigné et al., 2012; Blaudin de Thé and Lafourcade, 2016). Regarding air pollution, while its negative effects on human health have been well established (Chay and Greenstone, 2003, 2005; Currie and Neidell, 2005; WHO, 2016), the literature analysing the effects of transportation on air pollution is still inconclusive. Small and Kazimi (1995) estimate the cost of air pollution for an average automobile on the road in California in 1992 at 0.03 per mile, falling to half that amount in the year 2000. Gallego et al. (2013) study the effect of policies that persuade drivers to give up their cars in favour of public transport. They find that household responses to both policies they analyse induced more cars on the road and higher pollution levels. In the same line, Bel and Rosell (2013) find

[3] The average urban population density in the European metropolitan areas available from OECD in 2011 was 718 persons per km^2, compared to only 282 in the US.

[4] Car use in Europe is relatively low (about 42 percent lower than in US) (OECD and Eurostat).

[5] Europe is the world's leader in rapid transit systems. Based on Gonzalez-Navarro and Turner (2016), the number of subway km per inhabitant in European cities is more than double compared to North American cities (1.9 compared to 0.9 km per 1,000 inhabitants).

that the law that restricted the maximum speed in some of Barcelona's highways to 80km/h caused an *increase* on nitrogen oxides and particulate matter. On the other hand, Wolff (2014) analyse the Low Emission Zones (LEZ) in Germany and find substantial welfare benefits of such more 'drastic' policies. From the papers that focus on the city level, Heblich et al. (2016) find that historical pollution patterns induced neighbourhood sorting and within-city deprivation in the 19th century, an effect which persists up to now, while Hilber and Palmer (2014) find evidence that increasing car use *reduces* air pollution for a panel of 75 metro areas across the globe. From the literature on public transport, Gendron-Carrier et al. (2016) investigate the relationship between the opening of a city's subway network and its air quality. They find that particulate concentrations drop by about 4 percent following a subway station opening and that this effect seems to be very persistent over time. Finally, Fu and Gu (2017) find that the national toll waiver applied for an eight-day National holiday in China in 2012, increased pollution by 20 percent and decreased visibility by one kilometre.

Analysing all these effects for the whole of Europe is methodologically challenging. Finding valid instruments for such a big and heterogeneous area as Europe is complicated. The first contribution of this paper is that we combine GIS data for the Roman roads, the main trade routes during the Holy Roman Empire (15th century), the main post routes in 1810 and the railroad network in 1870 to obtain unbiased estimates for the 'fundamental law' elasticity. A second contribution of this paper is that we decompose the effect of the highway expansion to the effects of capacity and coverage expansion. While the former seems to drive most of the induced demand effect, the latter comprises the heart of the EU Cohesion Policy goals related to road infrastructure i.e. increasing cross-country and regional connectivity (TEN-T network).

The identification of the effect of highway traffic on urban air pollution is not straightforward. The level of emissions attributed to road transport decreased by 50 percent in the period 1985-2005, mainly as a result of the regulation regarding fuel quality and to other technological improvements. Our research design focuses on the effect of increasing highway supply *ceteris paribus* — i.e. keeping technology, regulation and other factors that potentially affected air pollution constant. Therefore, a third contribution is that we are able to isolate the effect of the fundamental law on air pollution addressing several endogeneity concerns, by using unique data of air pollution that are attributed to road transport *only*.

We also estimated the direct effect of highway development on urban air pollution in order to derive some back-of-the-envelope calculations of the cost of highway development in terms of air pollution. Based on these results, we estimated the cost of the additional air pollution to be about €6.3 million in the cities of our sample, as a result of the 1981-2001 highway construction. This is a *relative small* cost compared to the benefit of the aforementioned 50 percent reduction in road transport emissions during the same 20 year period. The cost of the increase in highway supply is only 2.43 of the monetary benefit of the actual reduction in emissions of air pollutants in the period 1985-2005.

Another important contribution of this paper is that we estimate the relationship between highway congestion and air pollution. Omitting the interaction effect between different externalities might have unexpected outcomes after the implementation of a policy. For example, Bento et al. (2014) demonstrate the first-order importance of the interaction effect between the introduction of the Clean Air Vehicle Stickers policy in California and unpriced congestion and show that it generates substantial welfare *losses*, dominating the expected primary welfare gain of the policy.

Finally, we study the heterogeneity of both effects (on congestion and air pollution) based on the existence of tolls and subways. In cities with tolls and in cities with subways, trafific congestion and urban air pollution decreased, as a result of the highway development. These findings have major implications for policy given the severity of traffic congestion and air pollution in Europe's cities.

The rest of this paper is organized in four sections and an Appendix. Section 3.2 describes the process of database construction and presents some descriptive statistics regarding the evolution of the highway network, highway traffic and air pollution in European cities. In Section 3.3, we analyse the 'fundamental law of highway congestion' in Europe's cities. In Section 3.4, we analyse the effect of highway traffic on urban air pollution and in Section 3.5, we highlight the most important findings and we draw our final conclusions and some policy recommendations. Finally, the Appendix includes a Data Appendix, the robustness analysis, the reduced form estimates of the direct effect of highway construction on air pollution and some maps that are discussed in the main text.

3.2 Data

3.2.1 Dataset construction

Apart from the population data, all the data that have been used in this paper are derived from maps using GIS software. Although this task involved a considerable amount of map processing (including geo-referencing, map vectorizations, manual network editing etc.), this data collection strategy allowed us to focus on the city level for the whole of Europe and for a long period of time (20 years). The units of our analysis are the Large Urban Zones (LUZ), as defined by Eurostat's Urban Audit in 2008. Eurostat defines LUZ not only based on their administrative and statistical unit borders but also in relation to commuting criteria, defining a functional urban area based on a perfectly harmonised methodology across Europe[6]. This definition comprises all the settlements that interact economically with the core (Arribas-Bel et al., 2011).

We are able to address many endogeneity concerns regarding the effects we want to estimate by means of our unique data, instrumental variables and panel data techniques. Specifically, we use the road traffic census from United Nations Economic Commission for Europe (UNECE), which contains detailed traffic and road infrastructure geographical information for every five years from 1985 to 2005. From the UNECE traffic census, we obtain information on the Average Annual Daily Traffic (AADT), the length, the number of lanes and the capacity of each segment (in terms of daily traffic). Multiplying segment length with AADT, we calculate the Vehicle Kilometres Travelled (VKT) for each highway segment. We also calculate the highway lane km as the product of the number of lanes and the length in km for each segment. We sum both VKT and highway lane km for each LUZ for each decade.

We have merged the UNECE traffic dataset to the evolution of the highway network with a 4-year lag, mitigating reverse causality concerns. For information on the highway and railway networks, as well as the subway lines in 2011, on the historical transportation networks, on geographical variables, current, past and historical population, we use the GIS maps and the database we created in Chapter 2. The highway infrastructure measures were calculated using GIS maps of the road system in Europe that form part of the *RRG* GIS Database[7]. The highway, railway and subway definitions used in this dataset follow their corresponding country definitions[8]. In order to construct our panel dataset for the highways and railways in each decade in the period 1981-2001, we used the RRG operational networks in each decade. We also use decennial data for the length of secondary and tertiary roads that we obtained from EC DG-REGIO (for more details, see Stelder (2016)).

We also use a very rich dataset of air pollutants, which includes emissions attributed *solely* to road transport. This measure provides the 'ideal' outcome variable for this analysis and helps us overcome many omitted variable and measurement error issues. EDGAR (Emissions Database for Global Atmospheric Research) is used

[6]Eurostat's LUZs approximate the *Functional Urban Area* (*FUA*) as defined by the OECD. The OECD and the European Commission developed a new harmonized definition of a city and its commuting zone in 2011. This new OECD-EC definition identified more than 800 cities with an urban centre of at least 50,000 inhabitants in the EU, Switzerland, Croatia, Iceland and Norway.

[7]Büro für Raumforschung, Raumplanung und Geoinformation (RRG) GIS Database.

[8]A general definition of a highways is a dual-carriageway designed for high-speed vehicular traffic while subways generally refer to metro or underground.

as the reference inventory of anthropogenic emissions, providing global grid maps of sector-specific historical emission data from 1970 to 2010 for direct greenhouse gases, ozone precursor gases, acidifying gases, primary particulates, as well as for mercury and for other stratospheric ozone depleting substances. EDGAR data are provided by the Institute for Environment and Sustainability (IES), Air and Climate Unit (European Commission - JRC Joint Research Centre). In particular, in this study we use the emissions of nitrogen oxides (NO_X), particulate matter (PM_{10}) and sulphur dioxide (SO_2), which are very harmful and highly associated to car use.

At high concentrations, these pollutants can have severe impacts on human health, including respiratory problems, resulting in escalating rates of premature human mortality (Beatty and Shimshack, 2014; EEA, 2012; Financial Times, 2013; Matus et al., 2012). They also damage ecosystems through the acidification and eutrophication of soil and water and act as important "climate forcers" (EEA, 2012).

Nitrogen oxides (NO_X) cause lung irritation and weaken the body's defences against respiratory infections, such as pneumonia and influenza. In addition, they assist in the formation of ground level ozone and particulate matter. Nitrogen oxides are emitted during fuel combustion, particularly by road transport, which consists about 50 percent of the total emissions in 2010 (EEA, 2012). There is evidence that the nitrogen dioxide fraction increased due to the high degree of diesel vehicles' penetration (up to 70 percent of NO_X) (Grice et al., 2009).

Sulphur dioxide (SO_2) can also cause respiratory problems and reduce lung function as well. Mortality and hospital admissions have been shown to increase on days with higher sulphur dioxide levels (WHO, 2008). Sulphur dioxide can also react in the atmosphere to form fine particles and poses the largest health risk to young children and asthmatics. Emissions of sulphur dioxide are predominately generated by the combustion of oil and coal. Yet, the contribution from road traffic is small and declining with the energy sector remaining the dominant emissions source (59 percent in 2010) (EEA, 2012).

Particulate matter of soot and metals give smog its murky color. Fine particles — less than one-tenth the diameter of a human hair (PM_{10}) — cause respiratory and cardiovascular diseases and pose the most serious threat to human health, as they can penetrate deep into lungs. Particulate matter causes direct (primary) pollution and secondary pollution from hydrocarbons, nitrogen oxides, and sulphur dioxides. Vehicles, power plants and various industrial processes generate substantial amounts of particulates while diesel exhaust is a major contributor to particulate matter pollution.

EDGAR v4.3.1 (version 4.3.1) is one of the few global emission inventories with consistent methodologies to calculate emission time series covering 4 decades for air pollutants with high spatial resolution of about 7.8 km^2 and consistent sector-specific breakdowns (Crippa et al., 2016). Recent comparisons show the reliability of this emission inventory based on the good agreement between the EDGAR 4.3.1 2008 and 2010 emission data and the best estimates provided by official national data merged with the EDGAR data set (Janssens-Maenhout et al., 2012). Emissions are calculated by taking into account activity data such as fuel consumption by sector, different technologies with installed abatement measures, uncontrolled emission factors and emission reduction effects of control measures. EDGARv4.3.1 relies on the annual international energy balances of the International Energy Agency (IEA) Statistics and regional or national information and assumptions on technology use and emission control standards[9]. Road transport emissions are calculated based on the types of vehicles included (heavy duty vehicles, light duty vehicles, passenger cars, buses, mopeds and motorcycles). The country specific fleet distribution dataset used is calculated based on registration, number of vehicles, driven vehicle kilometres from the International Road Federation (IRF, 2007) and historical data[10].

Emissions by country and sector were allocated on a spatial grid to provide a gridded emissions dataset for atmospheric modelling. To facilitate application of emissions data in local, regional and global modelling, a spatial grid of $0.1^\circ \times 0.1^\circ$ resolution (about 7.8 km^2) was built based on data such as location of energy and manufacturing facilities, road networks, shipping routes, human and animal population density and agricultural land use. Emission gridmaps are expressed in kg substance/m^2/s. Using this measurement unit, we calculated the mean for each LUZ. A screening of the available geographic datasets was performed for each emission source category with as main criteria coherent spatial coverage and reliability (EDGAR Methodology)[11].

The urban population dataset employed in this paper was constructed using census population figures collected every 10 years at the municipal level for the period 1961-2011 in 34 European countries, as provided by the DG REGIO of the European Commission. In spite of being one of the most solid and comprehensive statistical datasets available at the city level in Europe, Urban Audit suffers from many missing values (even in the population series), which means many of its variables are unsuitable for use. For this reason, we only adopt the delineation of the LUZ areas and use census data at the municipal level to construct our LUZ pop-

[9] The IEA dataset has been modified to adjust for incomplete time-series, geographical changes over time such as the former USSR.

[10] Incomplete time series, missing data in IRF were modified with statistics from Eurostat, UN-ECE transport statistics database (2008) and the Federal Office for Motorvehicles (KBA, 2007).

[11] For more details on the sectoral and spatial distribution of the EDGAR emission, see the Section 5.8.1 in the Appendix.

ulation dataset. This was a challenging task as it meant retrieving information for the numerous municipal mergers and changes in municipal codes from the national statistical offices. We used the LUZ population series for the period 1961-2001 and again, we merged it to the highway traffic and the air pollution data with a 4-year lag.

To compute our historical instruments, we worked with digital vector maps. For the 1810 post routes and for the 1870 railroads, we created our own GIS maps by geo-referencing and vectorizing the scanned maps from the David Rumsey Historical Map Collection[12] and from the Historical GIS for European Integration Studies[13] (see Figure 3.1 in Section 5.8.4), respectively. As a whole, there were more than 100,000 km of main and secondary roads in Europe. We also consider the Roman road network using the GIS map created by McCormick et al. (2005) and the length of the main trade routes in the Holy Roman Empire in the 15th century, computed based on Ciolek (2005) digital map. The map of the main trade routes in the 15th century includes, as its name indicates, the main routes between Central and Eastern cities[14]. As a whole, there were around 20,000 km of trade routes in Europe. The map of the Roman road network and the main trade routes in the Holy Roman Empire can be found in Figure 3.2 in Section 5.8.4.

We also include a number of historical variables in our analysis. The main historical variables used are dummy variables for the major cities in 800, 1000, 1200, 1450[15] and 1850 (Bairoch et al., 1988)[16]. In addition, we used a number of geographical variables, namely mean elevation, altitude range and the Riley et al. (1999) index of terrain ruggedness for each LUZ[17]. Another important geographical variable is the distance separating each LUZ centroid from the closest coastline.

3.2.2 Satellite VS ground VS EDGAR measures

A series of papers compare air pollution measures (mainly particulate matter) from satellites to measures from surface instruments (e.g. Gupta et al. (2006); Kumar et al. (2007)). Broadly, this literature concludes that satellite measures are good

[12]See `http://www.davidrumseny.com`.

[13]HGISE, see `http://www.europa.udl.cat/hgise`.

[14]e.g. Berlin (DE), Wien (AT), Warszawa (PL), Budapest (HU) or Zelenogradsk (RU)), but also with some other main European cities (e.g., Paris (FR), Basel (CH), Bruxelles (BE), Genova (IT) or Milano (IT).

[15]We created these variables from the maps contained in the Digital Atlas of Roman and Medieval Civilization.

[16]The European cities included in this dataset are those that had 5,000 or more inhabitants at any point between the 8th and the 18th centuries.

[17]The original GIS raster maps were downloaded from the Digital Elevation Model over Europe; see `http://www.eea.europa.eu/data-and-maps/data/eu-dem`.

proxies of airborne particulates, with two caveats. First, satellite reports describe daytime average conditions over a wide area and the time depends on the satellite's orbit, while ground based instruments record conditions at a particular location, often over a period of hours. This causes an obvious divergence between satellite and ground based measures. In addition, ground based instruments report the concentration of dry particulates, while the satellite based measure has trouble distinguishing water evaporation from other particles (Gendron-Carrier et al., 2016). Finally, satellite based measures evolve in concert with weather systems, as shown by changes in the winds and clouds (Al-Saadi et al., 2005).

While ground measures appear to be more accurate than satellite measures of air pollution, data availability from ground stations over a long period of time is scarce. On the other hand, EDGAR methodology seem to be a better way to measure air pollution as it is not sensitive to the time of measurement or to the specific location of the station. In addition, it is not affected by meteorological conditions and there is availability of annual and monthly sector-specific emissions on a spatial gridmap from 1970 onwards. The main limitation of EDGAR data is the accuracy of the spatial allocation of the emission data on a gridmap[18]. However, we test for the precision of EDGAR data using the air pollution data from Airbase to compare the measures of the three pollutants we analyse in this paper. Airbase is the European air quality database maintained by the European Environmental Agency (EEA) through its European topic centre on Air pollution and Climate Change Mitigation. Airbase data are collected from ground stations. However, the coverage before 2005 is very sparse in the Airbase dataset. Nonetheless, using 2005 as the year of comparison, we obtain a correlation coefficient between EDGAR and Airbase in the range of 0.3 for all three pollutants. Considering that the measures of Airbase are not restricted to road transport, this correlation seems very reassuring about the quality of the EDGAR database[19].

3.2.3 Descriptive Statistics

Table 3.1 presents some descriptive statistics for the analysis of the fundamental law of highway congestion. Table 3.1 highlights that Vehicle Kilometres Travelled (VKT) increased intensely compared to the lane km of the highway network. On the other hand, LUZ population increased considerably less than the highway lane

[18]The geographical database was build based on data such as the location of the road networks. The input datasets where point, line and area grids at various resolutions using GIS techniques for conversion, resampling and aggregation.

[19]The average share of road transport to the emissions of these three pollutants is about 20-25 percent when we consider the sectors which are more relevant in cities (residential and other buildings, road and non-road transport).

km. These statistics suggest that there is a clear positive correlation between VKT and highway lane km and that the induced demand effect on traffic is not driven by a substantial migration inflow to the cities of our sample.

Table 3.1: Average VKT, lane km and LUZ population per LUZ

	1985	1995	2005	1985-1995	1995-2005
Vehicles Kilometres Travelled (VKT)	2,441,112	3,289,878	4,206,788	34.77%	27.87%
Highway lane km	1,514	1,597	1,713	5.47%	7.27%
Population (LUZ)	452,974	468,684	483,490	3.47%	3.16%

Notes: Averages were calculated for our sample of 545 cities.
Source: Authors' own calculations based on data from UNECE and DG REGIO (EC).

Table 3.2: Average VKT, lane km and urban air pollutants per LUZ

	1985	1995	2005	1985-1995	1995-2005
Nitrogen oxides (NO_X)	3.81E-11	3.90E-11	2.66E-11	2.36%	-31.79%
Particulate matter (PM_{10})	4.96E-12	3.90E-11	4.30E-12	686.29%	-88.97%
Sulphur dioxide (SO_2)	5.54E-12	4.47E-12	3.01E-13	-19.31%	-93.27%
Vehicles Kilometres Travelled (VKT)	2,655,710	3,587,215	4,576,217	35.08%	27.57%
Highway lane km	1,659	1,758	1,884	5.93%	7.21%

Notes: Emission are expressed in average kg substance/m^2/s per LUZ. Averages were calculated for our sample of 545 cities.
Source: Authors' own calculations based on data from EDGAR and UNECE.

Table 3.2 presents some descriptive statistics at the city (LUZ) level of the evolution of transport-related emissions for the three pollutants that we focus our analysis. While the emissions of fine particulate matter (PM_{10}) rose immensely in the period 1985-1995, the emissions of all three air pollutants dropped significantly in 1995-2005. On average, air pollution attributed to road transport decreased by 48 percent for our sample of 545 cities. This reduction is mainly the result of European emission standards for passenger cars, which introduce different emission limits for diesel and petrol vehicles. On the other hand, during the whole period 1985-2005, highway Vehicle Kilometres Travelled (VKT) increased considerably. Thus, the sign of the relation between increased traffic and urban air pollution cannot be easily determined beforehand using simple descriptive analysis.

3.2.4 Emission technology and regulation

The largest effects of technology changes and end-of-pipe (EOP) control measures are observed in the road sector in the EU. In terms of regulation, already in the 1970s, Europe was moving towards the use of cleaner fuels, strengthened by the agreements made in the international Convention on Long-Range Transboundary Air Pollution (CLRTAP) and Gothenburg Protocol, thus reducing sulphur dioxide (SO_2) road emissions (EU Air Quality Standards). The first two stages of the European directives (Euro 1 and 2) introduced in 1992 and 1994, respectively, set limits to hydrocarbons (HC), nitrogen oxides (NO_X) and fine particulate matter (PM_{10}) emissions. This explains the big reduction in PM_{10} emissions in Table 3.2 during the period 1995-2005. NO_X decreased relatively less, probably as a result of diesel fuelled vehicles. In Euro 1 and 2, diesels had more stringent CO_2 standards but were allowed higher NO_X emissions. The reduction in SO_2 emissions since 1990 was achieved as a result of a combination of measures, including the impact of EU directives relating to the sulphur content of certain liquid fuels. In 1999, the European Union directive 1999/32/EC (1999) required the improvement of petrol and diesel fuel quality, lowering their sulphur content even further (Crippa et al., 2016).

3.3 Highway congestion in Europe's cities

3.3.1 Econometric framework

In this section, we introduce the empirical framework used to estimate the effect of the highway network expansion on the level of congestion. Increasing the supply of highways is expected to lower the cost of car use in the short run because of the increase in the overall highway capacity in a city, which decreases traffic congestion. However, this reduction in the major component of the cost of car use, might affect the travel decisions of individuals regarding the mode and quantity of travel. The 'fundamental law of highway congestion' suggests that the long term average effect of increasing the supply of roads will be that induced demand will bring the level of congestion back to its initial level.

In order to test this hypothesis, we estimate the effect of an increase in the logarithm of highway lane km on the Vehicle Kilometres Travelled (VKT) using OLS, IV and city fixed effects specifications. If the elasticity of VKT with respect to highway lane km is below one, then the average level of congestion decreased, a unit elasticity would indicate that congestion remained constant, while an elasticity above one would mean that congestion actually increased, on average. Our sample covers 545 Large Urban Zones (LUZ) from the EU28 countries (except for Cyprus and Malta), Norway and Switzerland in 1985, 1995 and 2005[20]. Our main specification is the following:

[20]As it was mentioned in Section 3.2.1, we merged the highway traffic data (basically VKT) to the highway construction (lane km) and population data with a 4-year lag. This way, we mitigate reverse causality concerns.

$$log(VKT)_{it} = \alpha + \beta log(lane\ km)_{it} + \gamma log(Pop)_{it} + \delta(Geography)_i + \\ + \zeta(Past\ pop)_i + \iota(Hist.\ pop)_i + \eta^t + \eta^{country} + \epsilon_{it} \tag{3.1}$$

where *i* is the city (LUZ) and *t* is the decade. $Geography$ is controlled by the logarithm of the LUZ area, a suburbanization index, which is the ratio of Central City (CC) area divided by the LUZ area, the mean and range of LUZ elevation, the mean surface ruggedness for each LUZ and the logarithm of the distance to the closest coast from each LUZ's centroid. $Past\ pop$ is the logarithm of LUZ population in 1960, 1970 and 1980. $Hist.\ pop$ is controlled by the inclusion of dummy variables for historical major cities in 814, 1000, 1200, 1450 and 1850. η^t and $\eta^{country}$ are decade and country fixed effects, respectively.

Specification (3.1) includes population dynamics, a series of geographical variables, decade and country fixed effects in order to mitigate omitted variable bias concerns. However, there might still be unobservable characteristics that affect both the highway network development and the changes in traffic. For example, a city-specific productivity shock might both affect the plan of the highway construction and increase urban transport flows. We use instrumental variables in order to address such endogeneity concerns. We use the log sum of the length of Roman roads, the 15th century trade routes, the 1810 post routes and the 1870 railroads in each LUZ as an instrument for the number of highway lane km. Such historical transportation networks are orthogonal with respect to most modern city outcomes once we control for urban geography and history. We use the total length of all these historical transportation networks because almost none of these networks spread out over the whole Europe[21]. The first-stage specification of Specification (3.1) is thus:

$$log(lane\ km)_{it} = \kappa + \lambda log(\sum hist.\ transport\ km)_i + \mu log(Pop)_{it} + \\ + \nu(Geography)_i + \xi(Past\ pop)_i + \pi(Hist.\ Pop)_i + \vartheta^t + \vartheta^{country} + \epsilon_{it} \tag{3.2}$$

We also estimate a city fixed effects specification, where we obtain identification from time variation within city, controlling for city-specific locational endowments (η^i) that are invariant in this 20-year period. The city fixed effects specification is the following:

[21]The main post routes in 1810 and the railroads in 1870 cover most of Europe. However, their coverage varies significantly in different parts of Europe.

$$log(VKT)_{it} = \alpha + \beta log(lane\ km)_{it} + \gamma log(Pop)_{it} + \eta^t + \eta^i + \epsilon_{it} \tag{3.3}$$

We also use Specification (3.3), which is our most conservative estimate, in our heterogeneous analysis, interacting our main independent variable, $log(VKT)_{it}$, with dummies for cities with (no) tolls and (no) subways. Finally, we decompose the effect of an increase in lane km into the effect of increased length and the increase in total capacity from the highway expansion. The specification of this decomposition is the following:

$$\begin{aligned} log(VKT)_{it} = \alpha + \chi log(highw.\ km)_{it} + \psi log(highw.\ capacity)_{it} + \\ + \gamma log(Pop)_{it} + \eta^t + \eta^i + \epsilon_{it} \end{aligned} \tag{3.4}$$

where highway km is the total length and highway capacity is the total capacity of the highway network in each city.

3.3.2 Results

As we explained in Section 3.3.1, we will estimate the effect of highway lane km on VKT using a two-stage least squares approach. The relevance of the historical transportation instruments can be shown in Table 3.3, where we report our first-stage estimations. Column [1] in Table 3.3 shows our most parsimonious specification where we regress the logarithm of highway lane km on the logarithm of the total length of the four historical transportation networks. Column [1] suggests that when we only control for the logarithm of city population, country and year fixed effects, the coefficient of the log sum of the historical transportation networks is 0.4. Thus, an increase in the historical transportation network by 10 percent is associated with an increase in the modern highway lane km by 4 percent. In Column [2], we also control for city area, a suburbanization index, which is the ratio of CC area divided by the LUZ area, mean and range of elevation, mean ruggedness and the log distance to the coast. All these control variables are statistically significant and their omission would mean that the first stage estimation were biased. When we include past and historical population in Columns [3] and [4], our results show that historical transport infrastructure is still a highly significant predictor of the modern highway network. Estimations for each year separately instead of a pooled panel yield similar results.

Table 3.3: First-stage: Historical roads and modern lane km

Dependent variable:	ln(lane km)			
	OLS	OLS	OLS	OLS
	[1]	[2]	[3]	[4]
ln(total length of historical transportation)	0.406^a	0.174^a	0.165^a	0.159^a
	(0.040)	(0.047)	(0.047)	(0.048)
ln(LUZ population)	0.289^a	0.192^a	0.440^c	0.420^c
	(0.036)	(0.036)	(0.233)	(0.235)
Geography		✓	✓	✓
Past population			✓	✓
Historical population				✓
Country fixed effects	✓	✓	✓	✓
Year fixed effects	✓	✓	✓	✓
Observations	1,635	1,635	1,635	1,635
R^2	0.608	0.663	0.670	0.672

Notes: The total length of historical transportation is the sum of the Roman roads, the 15th century trade routes, the 1810 post routes and the 1870 railroads in each LUZ. The sample used comprises 545 cities in 3 decades (1985-2005). Geography is controlled by the logarithm of the LUZ area, a suburbanization index, which is the ratio of CC area divided by the LUZ area, the mean and range of LUZ elevation, the mean surface ruggedness for each LUZ and the logarithm of the distance to the closest coast from the CC centroid. Past population is the logarithm of LUZ population in 1960, 1970 and 1980. Historical population is controlled by the inclusion of dummy variables for historical major cities in 814, 1000, 1200, 1450 and 1850. Robust standard errors are clustered by LUZ and are in parenthesis. ^{a}, ^{b} and ^{c} indicates significant at 1, 5, and 10 percent level, respectively.

After having established the relationship between historical transportation and modern highways, we present the results of Specification (3.1) in Table 3.4. Column [1] shows a naïve pooled panel specification, where we regress the logarithm of Vehicle Kilometres Travelled (VKT) on the logarithm of highway lane km, only including country and year fixed effects as control variables. The estimated elasticity of VKT with respect to highway lane km is 0.947 and highly statistically significant. This elasticity is roughly a unit elasticity, as in Duranton and Turner (2011) for the US and Hsu and Zhang (2014) for Japan. However, once we control for the logarithm of city population in Column [2], the estimated elasticity drops to 0.74. In Column [3], we also control for geographical variables, namely, the logarithm of the LUZ area, the suburbanization index, as well as past and historical population. The estimated coefficient of the log lane km becomes 0.83.

As we mentioned earlier, such estimates are subject to omitted variable bias. We address these concerns by means of instrumental variables. In Table 3.3, we demonstrated that historical transport infrastructure is a relevant instrument for modern highways. We also argued that this is a valid instrument since the exogeneity restriction is very likely to hold, once we control for geographical and historical variables. In Columns [4] and [5], we show the results of a two-stage least squares

(TSLS) estimation, following the OLS estimations shown in Columns [2] and [3]. The estimated elasticity using an instrumental variables approach is roughly one, once we include all geographical, past and historical population controls in Column [5]. The estimated coefficients in Column [3] and [5] are not statistically different, suggesting that the bias of an OLS estimate is limited. Therefore, we consider the specification in Column [3] as our preferred specification. We use this specification as our baseline specification in the robustness checks we describe in Section 5.8.2 in the Appendix. In Section 5.8.2, we run robustness checks a country-specific linear trend, clustering the standard errors at the country level, controlling for railway length and using a quadratic specification. Our results are virtually unchanged in all these tests.

Table 3.4: The effect of highways on traffic congestion

Dependent variable:	ln(VKT)							
	OLS	OLS	OLS	TSLS	TSLS	TSLS	TSLS	TSLS
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]
	1985-2005					1985	1995	2005
ln(lane km)	0.947^a	0.735^a	0.832^a	0.701^a	0.976^a	1.799^a	1.117^a	1.266^a
	(0.026)	(0.031)	(0.033)	(0.077)	(0.309)	(0.384)	(0.287)	(0.337)
ln(LUZ population)		0.312^a	0.745^a	0.331^a	0.679^a	1.037^c	0.435	1.098^a
		(0.026)	(0.205)	(0.047)	(0.244)	(0.535)	(0.426)	(0.249)
Geography			✓		✓	✓	✓	✓
Past population			✓		✓	✓	✓	✓
Historical population			✓		✓	✓	✓	✓
Country fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
Year fixed effects	✓	✓	✓	✓	✓			
Observations	1,635	1,635	1,635	1,635	1,635	545	545	545
R^2	0.834	0.863	0.883	-	-	-	-	-
First-Stage F-statistic	-	-	-	102.9	11.04	10.29	10.46	10.09

Notes: The sample used in Columns [1]-[5] includes 545 cities in 3 decades (1985-2005). Geography is controlled by the logarithm of the LUZ area, a suburbanization index, which is the ratio of CC area divided by the LUZ area, the mean and range of LUZ elevation, the mean surface ruggedness for each LUZ and the logarithm of the distance to the closest coast from the CC centroid. Past population is the logarithm of LUZ population in 1960, 1970 and 1980. Historical population is controlled by the inclusion of dummy variables for historical major cities in 814, 1000, 1200, 1450 and 1850. We instrument log(lane km) using the log sum length of the post routes in 1801, the railroads in 1870, the Roman roads and the trade routes in 15th century. Stock and Yogo (2005)'s 10 percent critical value is 16.4. Robust standard errors are clustered by LUZ and are in parenthesis. ^{a}, ^{b} and ^{c} indicates significant at 1, 5, and 10 percent level, respectively.

Finally, Columns [6]-[8] report the estimated elasticities using a two-stage instrumental variables approach for each year separately. When we estimate the effect of highway lane km on VKT for each year separately, the first-stage F-statistic is below the 10 percent critical values of Stock and Yogo (2005), but still above the Stock and Yogo (2005)'s 15 percent critical values and their 'rule of thumb' (F-statistic above 10). Column [6] suggests that the effect of highway lane km on VKT was considerably higher in 1985 compared to 1995 and 2005, which is shown in Columns [7] and [8], respectively.

Another way to deal with unobserved characteristics is using time variation to obtain identification. By means of city fixed effects, following Specification (3.3), we control for all the variables that are city-specific and do not change over time. In Column [1] of Table 3.5, we focus exclusively on the time variation of our panel dataset and we obtain similar results as in Table 3.4. An elasticity of 0.7 is slightly lower, albeit not statistically different from our main estimates. Thus, we can conclude that the causal effect of highway lane km on VKT is in the range 0.7-1. This elasticity suggests that highway construction during the period 1981-2001 did not effectively reduce highway congestion. In the long term, induced demand caused an almost proportional increase in traffic, which kept the level of congestion largely unchanged.

As a next step, we want to break down the effect of the increase in highway provision into a 'coverage effect' and a 'capacity effect' (Hsu and Zhang, 2014). In Column [2], we attempt to disentangle the two effects using the length and the total capacity of the highway network (expressed in terms of daily traffic) as separate regressors. Our estimates suggest that the effect of lane km on VKT is mainly driven by the increase in the total capacity of the highway network and less by the increase in the coverage of the network[22]. While total capacity seems to drive most of the effect we estimate, increasing cross-country and regional connectivity (or coverage) comprises the heart of the EU Cohesion Policy goals related to road infrastructure (TEN-T network).

In Columns [3] and [4], Table 3.5, we investigate whether our estimates are different in cities that apply (congestion) pricing schemes (tolls)[23] and in cities with rapid transit (subways). In Column [3], we interact the logarithm of highway lane km with a dummy variable for the cities where the highways are tolled in more than 25 percent of the total highway network in the city, which is the average percentage of tolls in the cities of our sample[24]. We also include another interaction term for the cities where the toll highways are less than the aforementioned threshold. 202 cities out of the 545 (about 37 percent) in our sample have tolls in more than 25 percent of their respective highway network. In Column [3], we find statistically different coefficients with a much higher effect in the cities without tolls. This result can be regarded as novel evidence in line with the recent literature, which suggests

[22]These results are even more accentuated when we use average capacity instead.

[23]Tolls in Europe are used to finance their construction and they are not usually related to congestion (Albalate and Bel, 2012).

[24]We have also used alternative percentages (above 0, 20 and 40 percent) and the results still hold.

that the solution to urban congestion is congestion pricing (Santos, 2004; de Palma et al., 2006; Winston and Langer, 2006; Leape, 2006; Eliasson and Mattsson, 2006). Finally, in Column [4], we interact our main variable of interest with a dummy variable for the the cities of our sample that have a subway system by 2011[25]. Duranton and Turner (2011) found no effect of public transit on VKT. However, in their application, they only used buses, which are as prone to traffic congestion as cars[26]. Using cities with subways instead, shows the response of the demand in cities with a fast alternative during congested times. We find a significantly lower VKT elasticity in the cities without subways, supporting the results of Anderson (2014), which he decribes as "the first robust empirical evidence indicating that transit generates large congestion relief benefits".

Table 3.5: Fixed effects estimation, effect decomposition and heterogeneity

Dependent variable:	ln(VKT)					
	OLS [1]	OLS [2]		OLS [3]		OLS [4]
ln(lane km)	0.717^a (0.098)		tolls*ln(lane km)	0.532^a (0.201)	subways*ln(lane km)	0.204 (0.393)
ln(length km)		0.300^a (0.094)	no tolls*ln(lane km)	0.847^a (0.139)	no subways*ln(lane km)	0.720^a (0.121)
ln(total capacity)		0.827^a (0.143)				
ln(LUZ population)	-0.008 (0.274)	-0.037 (0.272)	ln(LUZ population)	-0.050 (0.342)	ln(LUZ population)	-0.005 (0.337)
LUZ fixed effects	✓	✓	LUZ fixed effects	✓	LUZ fixed effects	✓
Year fixed effects	✓	✓	Year fixed effects	✓	Year fixed effects	✓
Observations	1,635	1,623	Observations	1,635	Observations	1,635
R^2	0.664	0.691	R^2	0.973	R^2	0.972

Notes: The sample comprises 545 cities in 3 decades (1985-2005). Column [2] includes 4 cities less because we lack highway capacity information in these cities. Tolls is a dummy variable which is one in the LUZ where more than 25 percent of their highway length has tolls (202 out of 545). Subways is a dummy variable which is one in the LUZ with subways in 2011. Robust standard errors are clustered by LUZ and are in parenthesis. ^{a}, ^{b} and ^{c} indicates significant at 1, 5, and 10 percent level, respectively.

[25]We use the year 2011, because of data availability restrictions. However, we acknowledge that this variable could be endogenous.

[26]Except if we consider bus lanes that enable buses to move faster.

3.4 Congestion and air pollution in Europe's cities

3.4.1 Empirical framework

In Section 3.3, we provided evidence that the 'fundamental law of highway congestion' holds for the cities of Europe. In this section, we will analyse the effect of the increase in highway traffic, caused by the expansion of the highway network, on urban air pollution. We use OLS and fixed effects to validate the robustness of our results[27]. Our main Specification (3.5) is presented below:

$$\begin{aligned} log(Pollutant)_{it} = \alpha + \beta log(VKT)_{it} + \gamma log(Pop)_{it} + \delta(Geography)_i + \\ + \zeta(Past\ pop)_{it} + \eta^t + \eta^{country} + \epsilon_{it} \end{aligned} \tag{3.5}$$

Where $log(Pollutant)$ is the logarithm of the average concentration of either nitrogen oxides (NO_X) or sulphur dioxide (SO_2) or particulate matter (PM_{10}) attributed to road transport in each city *i* and 5-year period *t*. Given that we have data for both emissions and VKT for 5-year periods, time fixed effects control for common unobserved changes in technology and regulation for every five years instead of ten years that we used in Section 3.3. $log(VKT)$ is the logarithm of the Vehicle Kilometres Travelled (VKT). We use VKT as our main variable of interest because the measurement of our dependent variables is based on fuel consumption per sector. Because of the changes in the quality of fuel as well as in car technology, an increase in the quantity of travel does not necessarily result in an increase in fuel consumption. However, in this study we are interested in the effect of the increase in highway traffic driven by the induced demand effect of the fundamental law. Therefore, using VKT as the main variable of interest seems more relevant than using the increase in the supply of highways. We also use the log(lane km) as the main variable of interest in Section 5.8.3 in the Appendix in order to derive some back-of-the-envelope calculations regarding the highway investments in Europe's cities.

Even though the use of a dependent variable which is attributed solely to our main variable of interest solves virtually most endogeneity concerns, we control for population and geographical variables. We control for past and current population in order to isolate the effect of highway traffic because of the highway development

[27] The use of IV in this specification is considered redundant given that our dependent variable is attributed to road transport. Therefore, omitted variable bias is not a concern here. It should be mentioned though that using the highway lane km as an instrument for VKT yields very similar results as the OLS specification.

on pollution from migration inflows [28]. The inclusion of geography is important because such factors have the potential to condition the concentration of pollution in city centres. For example, the physical and geographic characteristics of cities, have been identified in the literature as being strongly associated with urban air pollution (Hilber and Palmer, 2014). To control for geography, we use the same variables as in section 3.3, i.e. the logarithm of the LUZ area, a suburbanization index, which is the ratio of Central City (CC) area divided by the LUZ area, the mean and range of LUZ elevation, the mean surface ruggedness for each LUZ and the logarithm of the distance to the closest coast from the LUZ centroid.

An alternative source of identification is the use of a city fixed effects regression. Specification (3.6) below is based on the time variation of traffic and pollution within each city. Using the same specification, we conduct our heterogeneous analysis, interacting our main regressor, $log(VKT)_{it}$, with dummies for cities with (no) tolls and (no) subways.

$$log(Pollutant)_{it} = \alpha + \beta log(VKT)_{it} + \gamma (Pop)_{it} + \eta^t + \eta^i + \epsilon_{it} \tag{3.6}$$

In Section 3.2.4, we discussed the recent emission regulations related to road transport in Europe. These regulations were mainly EU policies. As such, their effect was similar in the whole EU after the introduction of each directive. Therefore, time fixed effects are expected to account for the effects of European regulation on air pollution. In addition, as we mentioned in section 3.2.1, EDGAR emissions are calculated by taking into account activity data such as different technologies with installed abatement measures, uncontrolled emission factors and emission reduction effects of control measures. These emission factors are country-sector and year specific. Therefore, any national divergence from EU regulations is also in principle incorporated in our dependent variable of emissions. However, there are also local regulations, such as Low Emission Zones (LEZ) and a few other Urban Access Regulations (Urban Road Tolls, Traffic Limited Zones and Traffic Restrictions). LEZ are areas —usually within cities and larger towns— with various restrictions on the operation of more polluting, typically older vehicles. Cities and governments have been adopting LEZ programs as a measure to reduce ambient exposures to air pollution in order to meet the EU Air Quality Standards. Such Environmental

[28] The inclusion of historical population is no longer meaningful because in this section, our dependent variable is urban air pollution and we do not use historical instruments. Past population is the logarithm of LUZ population in 1960, 1970 and 1980.

Zones started in Sweden in 1996, which can be considered the first LEZ program. Following the Swedish example, LEZ were implemented in a few cities in Germany, the Netherlands, north Italy, as well as London in 2007-2008. Based on the available data in `http://urbanaccessregulations.eu/`, for all countries except Sweden[29], Urban Access Regulations regulations were implemented after the end of our period of analysis.

3.4.2 Results

In Column [1], [3] and [5] of Table 3.6, we use an OLS specification, following our preferred specifications of Table 3.3 (Column [3]) i.e. including geographical variables, past population, country and year fixed effects. In Columns [2], [4] and [6], we use LUZ fixed as an alternative approach to control for city-specific factors that are invariant in 5-year time intervals. Using time fixed effects, our identification is based on time variation at the city level.

Table 3.6: NO_X, SO_2 and PM_{10} results

Dependent variable:	ln(NO_X)		ln(SO_2)		ln(PM_{10})	
	OLS	OLS-FE	OLS	OLS-FE	OLS	OLS-FE
	[1]	[2]	[3]	[4]	[5]	[6]
ln(VKT)	0.079^a	0.110^a	0.117^a	0.387^a	0.075^a	0.036
	(0.020)	(0.029)	(0.022)	(0.072)	(0.021)	(0.037)
ln(LUZ population)	0.974^a	-0.179	0.958^a	-0.689^b	1.038^a	0.347
	(0.026)	(0.148)	(0.027)	(0.308)	(0.027)	(0.241)
Geography	✓		✓		✓	
Country fixed effects	✓		✓		✓	
LUZ fixed effects		✓		✓		✓
Year fixed effects	✓	✓	✓	✓	✓	✓
Observations	2,720	2,720	2,720	2,720	2,720	2,720
R^2	0.847	0.358	0.855	0.846	0.886	0.878

Notes: The sample comprises 544 cities in five 5-year periods (1985-2005). Geography is controlled by the logarithm of the LUZ area, a suburbanization index, which is the ratio of CC area divided by the LUZ area, the mean and range of LUZ elevation, the mean surface ruggedness for each LUZ and the logarithm of the distance to the closest coast from the CC centroid. Past population is the logarithm of LUZ population in 1960, 1970 and 1980. Robust standard errors are clustered by LUZ and are in parenthesis. ^{a}, ^{b} and ^{c} indicates significant at 1, 5, and 10 percent level, respectively.

Columns [1]-[2] report the results for NO_X. Column [1] suggests that an increase in VKT by 10 percent causes a 0.79 percent increase in the concentration of NO_X at the city level. In Column [2], the estimated elasticity is 0.11 and highly statistically significant as well, suggesting that our results are very robust. Columns

[29]We created a dummy variable for Stockholm after 1995 to control for this local regulation.

[3]-[4] and [5]-[6] follow Columns [1]-[2] using SO_2 and PM_{10}, respectively, as the dependent variable instead of NO_X. While both OLS and fixed effects estimates in Columns [3] and [4] are relatively high and statistically significant, we treat the estimated elasticity value with caution, due to the differences between the two specifications. However, since both estimates are consistent in the direction and the statistical significance of the effect, we may conclude that traffic congestion increased the emissions of SO_2 considerably in Europe's cities (SO_2 elasticity to VKT of at least 0.12). Finally, Columns [5]-[6] provide the estimates of the same specifications for PM_{10}. While the OLS and fixed effects estimates in Columns [5] and [6] are positive, the fixed effects estimate is lower and not statistically significant. Thus we consider that the positive effect of increased traffic on the emissions of fine particulate matter is only tentative.

One concern regarding this estimation is that some of the effect on pollution that we measure could be driven by a displacement effect between highways and other non-highway roads. In order to deal with this concern and in order to derive some back-of-the-envelope calculations regarding highway investments, Table 3.9 in Section 5.8.3, reports a reduced form estimation of the direct effect of highway lane km on air pollution. Using this alternative specification, we estimated an elasticity, which is highly statistically significant and approximately 0.1 for all NO_X, SO_2 and PM_{10}. In Table 3.9, we also control for the logarithm of secondary and tertiary road length and we find that such roads had no effect on air pollution.

Using the direct estimates of highway lane km on air pollution from Table 3.9, we can derive some back-of-the-envelope calculations of the cost of emissions attributed to the construction of highways. Using these estimates and the valuation of the three different pollutants that we analyse in Muller and Mendelsohn (2009), we calculate the economic cost of the highway network expansion in the cities of our sample during the period 1981-2001. Based on these estimates, the cost of pollution because of the highway development is about €6.3 million in the period 1981-2001, which is arguably a limited effect. To put this figure in perspective, we calculate the benefit of the total reduction in emissions of air pollutants attributed to road transport during the same period (about 50 percent on average). The monetary benefit of this reduction is about €261.3 million in these 20 years. In other words, the cost of increasing the supply of highways is only 2.43 percent compared to the benefit of the actual improvements in fuel technology and regulation introduced in this period. Therefore, our results suggest that the reduction in emissions because of emission regulations and technological improvements outweigh by a great amount the positive effect of highway development on urban air pollution,

As in Section 3.3, we also investigate the effect of the increase in highway traffic in cities with tolls and in cities subways in Table 3.7. In all Columns [1]-[6], we use the OLS city fixed effects specification in order to obtain identification. In Columns [1]-[3], we include two interaction terms of the log VKT, for the cities with toll highways and for the cities without toll highways[30]. In all Columns [1], [2] and [3], we find a highly statistically significant and positive effect of highway traffic on NO_X, SO_2 and PM_{10}, respectively, while the coefficient for the cities with tolls is negative in all specifications. The negative coefficients in Columns [1]-[3], Table 3.7 suggest that an increase in traffic in cities with tolls decreases the level of urban air pollution. This result can be better interpreted if we think in terms of highway congestion. Our heterogeneous results in Table 3.5 suggest that traffic increased significantly less in cities with tolls. The estimated elasticity of VKT for the cities with tolls was approximately 0.5. Therefore, the average level of congestion in these cities decreased significantly. Consequently, we can interpret the negative coefficient of Columns [1]-[3], Table 3.7, as follows. A decrease in highway congestion (cities with tolls) reduces urban air pollution. On the other hand, the elasticity of air pollution with respect to VKT for the cities without tolls is very high. These estimates suggest that increasing the provision of highways without applying some form of congestion pricing might have detrimental consequences for the quality of air in a city.

Table 3.7: NO_X, SO_2 and PM_{10} heterogeneous results

	cities with tolls				cities with subways		
Dependent variable:	$\ln(NO_X)$ [1]	$\ln(SO_2)$ [2]	$\ln(PM_{10})$ [3]	Dependent variable:	$\ln(NO_X)$ [4]	$\ln(SO_2)$ [5]	$\ln(PM_{10})$ [6]
tolls*ln(VKT)	-0.066	-0.160^b	-0.205^a	subways*ln(VKT)	-0.095	-0.067	-0.206
	(0.047)	(0.081)	(0.052)		(0.164)	(0.241)	(0.149)
no tolls*ln(VKT)	0.192^a	0.641^a	0.148^a	no subways*ln(VKT)	0.114^a	0.396^a	0.041
	(0.040)	(0.099)	(0.052)		(0.033)	(0.082)	(0.042)
ln(LUZ population)	-0.277^c	-0.993^a	0.213	ln(LUZ population)	-0.172	-0.674^c	0.355
	(0.163)	(0.329)	(0.265)		(0.165)	(0.344)	(0.270)
LUZ fixed effects	✓	✓	✓	LUZ fixed effects	✓	✓	✓
Year fixed effects	✓	✓	✓	Year fixed effects	✓	✓	✓
Observations	2,720	2,720	2,720	Observations	2,720	2,720	2,720
R^2	0.963	0.909	0.950	R^2	0.962	0.904	0.949

Notes: The estimates presented in Columns [1]-[9] include 544 cities in 3 decades (1985-2005) while Columns [3], [6] and [9] include the same number of cities during the same period in 5 year intervals. Robust standard errors are clustered by LUZ and are in parenthesis. ^{a}, ^{b} and ^{c} indicates significant at 1, 5, and 10 percent level, respectively.

Columns [4]-[6] in Table 3.7 show heterogeneous estimates of the effect of highway traffic on urban air pollution, where we interact the log VKT with a dummy

[30] Again, these are cities where more than 25 percent of the total highway network in the city is tolled (about 37 percent of the cities in our sample).

variable for the the cities of our sample that have a subway system by 2011[31]. Following our findings for the level of congestion, air pollution increased only in the cities without subways. The estimated coefficients for the cities with subways in Columns [4]-[6] are approximately the same as the coefficients for the whole sample in Table 3.6. On the other hand, the coefficients for the cities with subways are negative, although not statistically significant[32]. These results are in line with the work of Gendron-Carrier et al. (2016), who found a significant 4 percent reduction in the concentration of particulate matter in a 10km disk surrounding the city centres during the year following a subway opening. In addition, their results seem to be very persistent over time. Our results provide more arguments in favour of rapid transit provision policies.

3.5 Conclusions

In this paper, we provide evidence that the 'fundamental law of highway congestion' holds for the cities of Europe and we estimate the elasticity of Vehicle Kilometres Travelled (VKT) with respect to highway lane km to be in the range of 0.7-1. This result suggests that highway construction induced the demand for car travel almost proportionally, thus the level of congestion remained roughly unchanged on average in the period 1985-2005. We also decompose this effect into the effect of the increased coverage (length) of the network and the effect of the increase in the average highway capacity. Our estimates suggest that the induced demand effect was mainly driven by the total capacity expansion rather than the increased coverage of the network. As mentioned in Section 5.1, EU sponsored a considerable part of this highway development. One of the main goals of this policy was to increase cross-country and regional connectivity (TEN-T network). Thus, EU focused on improving the coverage of the highway network rather than on capacity expansions. Therefore, our results seem to provide supportive evidence for the highway investments of EU Cohesion Policy.

The second part of the paper shows that the increase in highway traffic (because of the highway development) caused a significant increase in the urban concentration of three air pollutants that pose at risk the health of urban dwellers. Specifically, the elasticity of nitrogen oxides (NO_X) with respect to VKT is approximately 0.1

[31] We use the year 2011, because of data availability restrictions. However, we acknowledge that the subway dummy could be endogenous.

[32] A positive effect between car use and pollution is not a novel finding. Hilber and Palmer (2014), who focus on a panel of 75 big cities around the globe find similar results. The authors tried to explain this negative effect through many different channels. However, subways and tolls were not among them. Therefore, the finding that an increase in car use in the cities with highways can decrease the concentration of air pollutants could be used as an interpretation of their results.

or in other words, an increase in traffic by 10 percent causes an increase in the concentration of nitrogen oxides by 1 percent. We also find evidence of a significant positive effect of VKT on the concentration of sulphur dioxide (SO_2), although our point estimates using different approaches differ in magnitude. As for the positive effect on particulate matter (PM_{10}), our results are suggestive but not robust enough to be conclusive.

Therefore, the results of this paper suggest that highway development in European cities has contributed to air pollution while it was not able to relieve traffic congestion. However, our back-of-the-envelope calculations suggest that the cost of air pollution caused the highway development during this 20-year period is about €6.3 million in the period 1985-2005, which is not substantial considering that the total benefit of the decrease in urban emissions attributed to road transport was €261 million during these years. Moreover, our heterogeneous analysis shows that the cities with tolls and the cities with subways experienced a lower increase in highway traffic and a lower effect on urban air pollution because of the highway development. These last results also suggest that a decrease in traffic congestion decreases air pollution.

These findings have major implications for policy given the severity of traffic congestion and air pollution in Europe's cities. First of all, they show that EU investments in highways did not augment air pollution in Europe's cites considerably, although they did not effectively relieve traffic congestion. Therefore, this study provides a positive evaluation of the EU Cohesion Policy in terms of the air pollution externality. In addition, pricing the use of highways can reduce traffic congestion and thus, air pollution, after a highway improvement. This is the case even if most tolls in European highways were not directly intended to internalise congestion. Moreover, rapid transit systems seem to be an effective way to moderate the negative externalities of road transport, arguably because they provide a high-speed and congestion-free alternative, which does not require car ownership, to commuters in cities. Subways, are much more common in Europe than in any other region of the world. The findings of this paper provide a positive evaluation of the past investments in public transportation in Europe and they suggest that the current EU policies that incentivise public transit (either through investments and improvements or through subsidising fare prices) are in the right direction.

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3.6 Appendix

3.6.1 Data Appendix: EDGAR sectoral and spatial allocation

The EDGAR data sets are calculated using a consistent bottom-up approach with full time series of the activity data. Emissions (EM) for a country *c* are calculated for each compound *x* on an annual basis (*t*) and sector wise (for *i* sectors, multiplying on the one hand the country-specific activity data (AD), quantifying the human activity for each of the *i* sectors, with the mix of *j* technologies (TECH) for each sector *i*, and with their abatement percentage by one of the *k* end-of-pipe (EOP) measures for each technology *j*, and on the other hand the country-specific emission factor (EF) for each sector *i* and technology *j* with relative reduction (RED) of the uncontrolled emission by installed abatement measure *k*, as summarized in the following formula:

$$EM_{c,i}(t,x) = \sum_{i,j,k} [AD_{c,i}(t) * TECH_{c,i,j}(t) * EOP_{c,i,j,k}(t) * \\ * EF_{c,i,j}(t,x) * (1 - RED_{c,i,j,k}(t,x))] \tag{3.7}$$

For the spatial distribution of the EDGAR emissions data, EDGAR On Line Open access (EOLO) system disposes over an extensive set of global proxy data that are representative for major source sectors. Emission sources are, depending on the source sector or subsector, considered either as diffuse or as point source. The diffuse sources are distributed over the grid cells with the proxy data covering the globe entirely or partially, whereas the point sources are allocated to points within a grid cell. In order to make both additive, the point sources are smeared out over the corresponding grid cell and their value is corrected by a geographical fraction such that the sum of the discrete grid cell values for a given (sub)sector corresponds to the country-specific total of that sector (Janssens-Maenhout et al., 2012). A screening of the available geographic datasets was performed for each emission source category with as main criteria coherent spatial coverage and reliability (EDGAR Methodology). Emission gridmaps are expressed in kg substance/m^2/s. Using this measurement unit, we calculated the mean for each LUZ.

3.6.2 Robustness checks for the fundamental law

In Section 3.3.2, we concluded that the bias introduced by an OLS is limited compared to the IV results. Therefore, in this section, we present some robustness checks using Specification (3.1) and Column [3] in Table 3.4, as our baseline specification. In our baseline OLS specification, we control for the log LUZ population, geographical variables, past and historical population, country and year fixed effects. We extend this specification in order address some concerns regarding our baseline results.

One first concern is that the standard errors are correlated beyond the city level that we cluster them in all our specifications. In order to address this concern, in Column [1], we cluster the standard errors by country. The standard errors do not rise considerably compared to our baseline specification. Another concern could be that a country may have experienced a shock which affected the development of the highways at the country level and the Vehicle Kilometres Travelled (VKT) at a specific time. Ideally, we would like to include country-specific decade fixed effects. However, following this approach, the degrees of freedom decrease substantially and the standard errors rise excessively. As an alternative approach, in Column [2], Table 3.8, we include a country-specific linear trend. The results do not change.

Table 3.8: Robustness tests for the main results

Dependent variable:	ln(VKT)						
	OLS	OLS	OLS		OLS		OLS
	[1]	[2]	[3]		[4]		[5]
ln(lane km)	0.831^a	0.833^a	0.825^a	ln(highw. lane km)	0.833^a	ln(lane km-$\overline{lane\ km}$)	0.814^a
	(0.034)	(0.056)	(0.035)		(0.034)		(0.037)
ln(rail km)			0.026^c	ln(secondary km)	-0.000	(ln(lane km-$\overline{lane\ km}$))^2	-0.014
			(0.015)		(0.004)		(0.017)
				ln(tertiary km)	0.002		
					(0.005)		
ln(LUZ pop.)	0.837^a	0.776^a	0.802^a	ln(LUZ pop.)	0.737^a	ln(LUZ pop.)	0.781^a
	(0.226)	(0.213)	(0.207)		(0.223)		(0.207)
Geography	✓	✓	✓		✓		✓
Past population	✓	✓	✓		✓		✓
Historical population	✓	✓	✓		✓		✓
Country fixed effects	✓	✓	✓		✓		✓
Year fixed effects	✓	✓	✓		✓		✓
Country-specific trend		✓					
Observations	1,635	1,635	1,635		1,635		1,635
R^2	0.886	0.883	0.883		0.883		0.883

Notes: $\overline{lane\ km}$ is the average lane km. The estimates presented in The sample comprises 545 cities in 3 decades (1985-2005). Geography is controlled by the logarithm of the LUZ area, a suburbanization index, which is the ratio of CC area divided by the LUZ area, the mean and range of LUZ elevation, the mean surface ruggedness for each LUZ and the logarithm of the distance to the closest coast from the CC centroid. Past population is the logarithm of LUZ population in 1960, 1970 and 1980. Historical population is controlled by the inclusion of dummy variables for historical major cities in 814, 1000, 1200, 1450 and 1850. Robust standard errors are clustered by LUZ and are in parenthesis. ^{a}, ^{b} and ^{c} indicates significant at 1, 5, and 10 percent level, respectively.

Column [3] includes the log of railway km to test for the effect of the most popular form of public transportation on highway congestion. Rail is considered as the main alternative to car travel. In Column [3], we include the log of railway length in each decade as an additional control variable. the estimated elasticity is rather low and only statistically significant at the 10 percent level. However, one might argue that this variable could be endogenous. In order to take into account at least reverse causality concerns regarding the endogeneity of the rail variable, we have also used log railway length in 1981 instead (not reported in the paper). Using the log rail length in 1981 yields an even lower and not statistically significant rail coefficient. These results suggest that railway development did not markedly affect highway traffic.

Another potential concern is that the increase in highway traffic and the highway development is affected by the supply of other roads that are not classified as highways. In Column [4], we add the log length of secondary and tertiary roads[33] in our baseline specification. The results suggest that such roads have no effect on VKT.

Finally, we also have to test for the functional form of the effect under study. One might expect that the effect of highway development on traffic congestion depends crucially on the extent of the highway network in each city. However, we cannot directly estimate a quadratic effect using our log level variables because of the high correlation between the linear and the quadratic variables. Therefore, we demean our main variable of interest, ln(lane km), by subtracting its mean value from each observation and then, we calculate its logarithm and the square of the latter. The quadratic term is not statistically significant and its value is very close to zero. Therefore, the log-log specification we use seems to be the correct specification to estimate this effect.

3.6.3 Reduced form and robustness results for air pollution

As we discussed in Section 3.4.1, we use Vehicle Kilometres Travelled (VKT) as the main variable of interest in Section 3.4.2 because we want to capture the intensity of car use, which is not necessarily captured in our measure of air pollution. However, estimating the elasticity of the different air pollutants with respect to the extensions of the highway networks is useful for policy recommendations. In Columns [1], [3] and [5], Table 3.9, we perform a reduced form estimation of the direct effect of highway lane km on the three different air pollutants. In addition, as discussed in Section 3.4.2, we are concerned about possible displacement effects between highways and other non-highway roads. In order to deal with this such concerns, in Columns [2], [4] and [6], we also control for the logarithm of secondary and tertiary

[33]Based on data from EC DG-REGIO, the average speed in highways is 97km/h while in secondary and tertiary roads is 76 and 54km/h, respectively.

road length.

The reduced forms for all pollutants in Columns [1], [3] and [5] report an elasticity which is approximately equal to 0.1. Therefore increasing the highway network by 10 percent causes an increase in air pollution of 1 percent, which is a substantial effect. In addition, Columns [2], [4] and [6] suggest that secondary roads had absolutely no effect on the concentration of any of the three pollutants. Finally, tertiary roads seem to have affected significantly the emissions of NO_X and SO_2. However, the estimated coefficient is rather small. Therefore, we can conclude that any displacement effect from other roads is expected to be minimal.

Table 3.9: Reduced form results for NO_X, SO_2 and PM_{10}

Dependent variable:	$\ln(NO_X)$		$\ln(SO_2)$		$\ln(PM_{10})$	
	OLS [1]	OLS [2]	OLS [3]	OLS [4]	OLS [5]	OLS [6]
ln(lane km)	0.095^a (0.028)	0.096^a (0.029)	0.116^a (0.032)	0.111^a (0.033)	0.096^a (0.029)	0.098^a (0.030)
ln(secondary km)		-0.003 (0.004)		0.008 (0.005)		-0.005 (0.005)
ln(tertiary km)		0.012^a (0.005)		0.019^b (0.008)		0.007 (0.005)
ln(LUZ population)	0.853^a (0.170)	0.854^a (0.168)	0.427 (0.287)	0.394 (0.288)	1.239^a (0.204)	1.248^a (0.203)
Geography	✓	✓	✓	✓	✓	✓
Past population	✓	✓	✓	✓	✓	✓
Country fixed effects	✓	✓	✓	✓	✓	✓
Year fixed effects	✓	✓	✓	✓	✓	✓
Observations	1,632	1,632	1,632	1,632	1,632	1,632
R^2	0.836	0.838	0.846	0.847	0.892	0.892

Notes: The sample comprises 544 cities in 3 decades (1985-2005). Geography is controlled by the logarithm of the LUZ area, a suburbanization index, which is the ratio of CC area divided by the LUZ area, the mean and range of LUZ elevation, the mean surface ruggedness for each LUZ and the logarithm of the distance to the closest coast from the CC centroid. Past population is the logarithm of LUZ population in 1960, 1970 and 1980. Robust standard errors are clustered by LUZ and are in parenthesis. ^{a}, ^{b} and ^{c} indicates significant at 1, 5, and 10 percent level, respectively.

3.6.4 Maps

Figure 3.1: Railways in 1870.

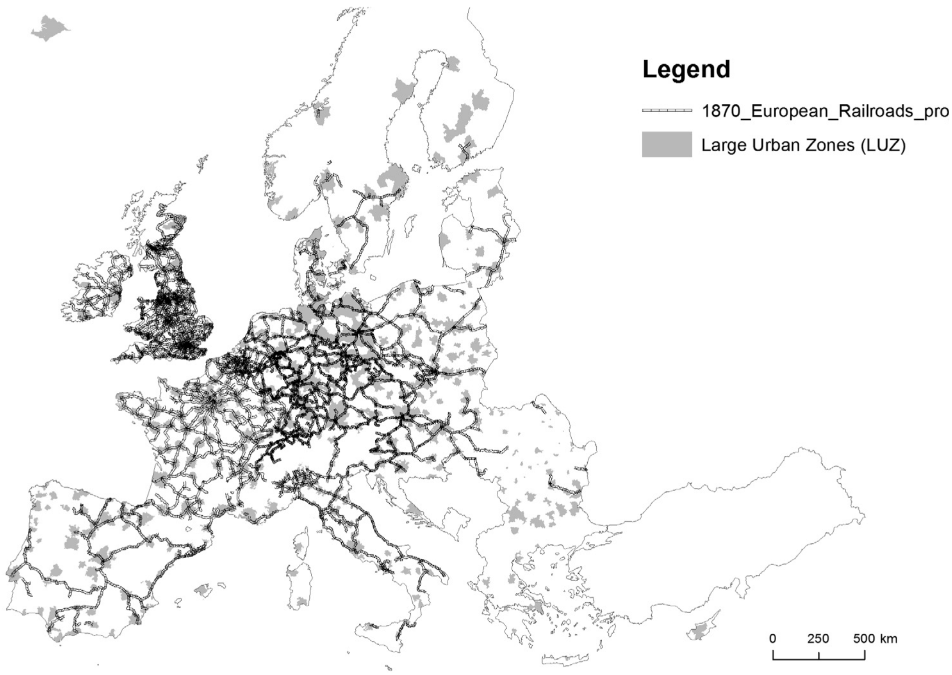
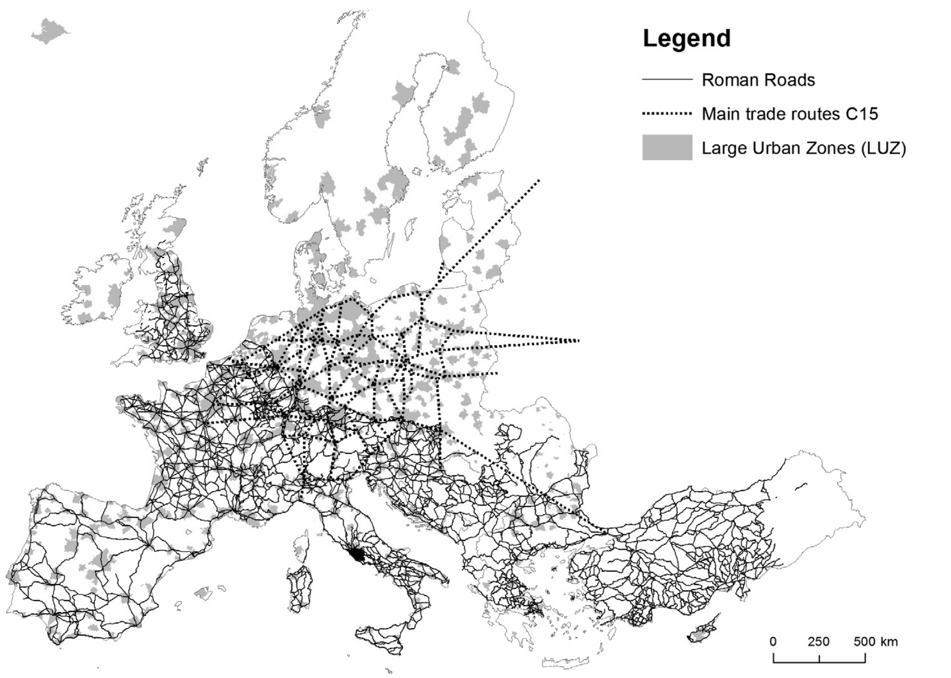


Figure 3.2: Roman roads and main trade routes during the Holy Roman Empire (C15).



4 Congestion by accident? A two-way relationship for highways in England.

4.1 Introduction

Traffic congestion and road accidents are considered the most important sources of external costs related to car travel (Shefer and Rietveld, 1997). Traffic congestion is an omnipresent phenomenon during rush hour in densely-populated areas (see, for example, Arnott and Small (1994); Downs (2005)). Congestion is an important problem for road transport and a main challenge for transport policy at all levels. The cost of traffic congestion for Europe is about 1 percent of the European GDP every year (Christidis and Ibáñez Rivas, 2012) and its mitigation is the main priority for most infrastructure, traffic management and road charging measures.

Congestion typically occurs at times of high travel demand or as a consequence of accidents and other non-recurring incidents that temporarily reduce road capacity. Non-recurrent congestion on highways is primarily caused by road accidents and other types of incidents (e.g., object on road, car breakdown) (Adler et al., 2013). This type of congestion typically constitutes roughly one-quarter of highway congestion (Snelder et al., 2013). Besides the impact of accidents on congestion, several thousands of people lose their lives and millions get injured as a result of road accidents. The total annual costs for society according to the valuation of accidents presented in the COWI (2006) report, which conducted an economic cost-benefit analysis for the DG-TREN of the European Commission, was estimated at €229 billion per year. Therefore, a rough approximation of the sum of traffic congestion and accident cost for the European Union would be close to 3 percent of the European GDP.

The goal of this paper is to estimate the causal effect of accidents on traffic congestion and *vice versa*. If a positive relationship between the two externalities is identified, policies that aim at reducing either of these issues will have multiplicative benefits. For instance, only recently was found that the introduction of London congestion charge not only reduced traffic congestion (Transport for London, 2003; Leape, 2006) but also had a significant effect on the number of road accidents and on the number of fatalities (Noland et al., 2007; Green et al., 2016). Such evidence suggests that there is a tendency to consider traffic congestion and accidents in isolation, rather than as two highly inter-dependent phenomena.

While many scholars have studied the effect of traffic congestion on road accidents since the '70s (Vickrey, 1968, 1969; Dickerson et al., 2000; Noland and Quddus, 2005; Quddus et al., 2010), only limited attention has been paid on the inverse relationship. The main issue that impedes such analyses has been data availability and the inherent endogeneity of this relationship. Road accidents typically occur in high congestion times. At the same time, accidents cause traffic congestion (Vitaliano and Held, 1991; Skabardonis et al., 2008; Elvik et al., 2004; Kwon and Varaiya, 2005; Adler et al., 2013). Moreover, both congestion and accidents are affected by several observable and unobservable factors (e.g. weather, road condition, speed limits, construction works, holidays or big events). Such factors could raise endogeneity concerns, suggesting that the identification of a causal relationship between road congestion and road accidents is a non-trivial issue.

The existing literature has mentioned some of these endogeneity concerns, albeit these issues have not always been addressed adequately. This paper estimates the effect of an accident's occurrence on the average flows, speeds and journey times using the observed patterns of traffic flows in England's highways in the period 2012-2014. Inspired by a panel data methodology that has previously been used to analyse electricity day-ahead market prices (Huisman et al., 2007) and the work of Adler et al. (2013), I take advantage of the stable periodic patterns of road traffic and the richness of information in the big traffic dataset in order to estimate the causal effect of accidents on traffic congestion.

The results of this study suggest that the delay caused by an accident is on average about 6.4 seconds per vehicle per kilometre travelled (s/vh/km). This effect could be translated to a 17.8 percent increase of the average journey time, which is a considerable effect. While the average speed reduction caused by an accident is also considerable (7.8 km/hour), I only find minor effects of an accident on traffic flows. For both journey times and average speeds, the effect of an accident on congestion declines sharply after the first 15-minutes[1]. The decay of the effect is 70-75 percent lower after the first quarter of an hour. When recurrently congested highway segments are considered[2], the effect of an accident on average journey time is 21 percent higher, compared to the case where the whole network is considered.

[1] Traffic congestion is defined here as the increase in the journey time.

[2] Defined as the segments that at each particular time of the day and day of the week, the monthly average speed is below 100km/h. I have also used alternative speed thresholds.

Finally, I find no evidence of rubbernecking[3].

These results are confirmed using simple differences and differences-in-differences estimations with a very reduced sample of the big dataset (using about 0.5 percent of the observations). This is evidence that large part of the information contained in big data could sometimes be redundant, whereas refining the meaningful information is the real challenge of 'big data'. It should be stressed that this is one of the few studies that uses a small portion of the increasing volume of big datasets, which becomes available from governments and local authorities worldwide. This can be regarded as an important contribution to the economics literature in general since until recently, economists have been reluctant to use "big data" in academic research (Varian, 2014).

Regarding the inverse effect (i.e. effect of highway congestion on the probability of an accident), I use dynamic panel data techniques in combination with a research design that makes use of the accidents that happened in 'good conditions' and dynamic panel data techniques. My estimates suggest that a 10 percent increase in journey time decreases the probability of an accident by 0.15 percent or in other words, a 16 percent of the average accident rate. Therefore, highly congested segments are associated with less accidents. This relation between the traffic variables and the probability of an accident is estimated to be convex.

This paper is structured as follows. Section 4.2 describes the data used and presents some descriptive statistics. In Section 4.3, I explain the identification and I introduce the econometric framework and the different specifications used in Section 4.4, where the estimation results are presented and discussed. Finally, Section 5.7 concludes the analysis of highway congestion and accidents.

4.2 Data and Descriptive Statistics

This paper uses very detailed data on highway traffic and accidents for England that are publicly available from the Highway Agency in the open data portal of the UK government `data.gov.uk`. These data have never been used before in an academic paper based on my knowledge. Sometimes, it is the size of such big datasets that is considered an issue but in most of the cases it is the detail of their information that is regarded as superfluous. However, the volume of information in the highway traffic dataset reveals some interesting patterns that allow the identification of the causal effect of highway accidents on traffic speeds and vice versa.

The Highways Agency network journey time and traffic flow data series provide

[3]'Rubbernecking' refers to road users driving at the other direction of the highway where the accident took place who 'rub their neck' in order to view the aftermath of a traffic accident.

traffic flow[4], average speed and journey time information for 15-minute periods from April 2009 to the mid-2015 on all motorways and most 'A' roads managed by the Highways Agency, known as the Strategic Road Network, in England. Average speeds and journey times[5] are estimated using a combination of sources, including Automatic Number Plate Recognition (ANPR) cameras, in-vehicle Global Positioning Systems (GPS) and inductive loops built into the road surface. The data includes a data quality indicator showing the quality of the journey time data for the link and time period. See below for detailed description:

1 = Observed or vertically[6] in-filled data with a good spatial match[7] to the link.
2 = Observed or vertically in-filled data with a poor spatial match to the link.
3 = Horizontally[8] in-filled data with a good spatial match to the link.
4 = Horizontally in-filled data with a poor spatial match to the link.
5 = No observed data so data are in-filled using free-flow data.

The accidents dataset provides detailed information about the circumstances of personal injury road accidents in Great Britain from 2005 onwards, the types of vehicles involved and the consequential casualties. Specifically, it includes information about road class, road surface, lighting conditions, weather conditions, casualty class, casualty severity, sex of casualty, age of casualty and type of vehicle. The statistics relate only to personal injury accidents on public roads that are reported to the police, and subsequently recorded, using the STATS19 accident reporting form. Information on damage-only accidents, with no human casualties or accidents on private roads or car parks are not included in this data. Hence, I only observe a proportion of the total number of accidents. However, the cost of such accidents was estimated by the Department of Transport to be at least ten times greater than property-damage-only accidents (Department of Transport, 1993). Moreover, where personal injury does occur, property damage is also likely to be more severe. However, it is important to keep the distinction in mind, especially when comparing my results with those of previous studies such as Vitaliano and Held (1991), who have a record of all accidents on their road segments (Dickerson et al., 2000).

[4] An average of the observed flow for the link, time period and day type.

[5] Note that journey times are derived from real vehicle observations and imputed using adjacent time periods or the same time period on different days.

[6] Vertical in-filling uses observed journey time data from adjacent time periods on the same day and link.

[7] Spatial match measures how precisely the source data maps onto the particular road link. For example, a pair of ANPR cameras that covered only a small portion of a complete junction-to-junction link may be reported as having a poor spatial match.

[8] Horizontal in-filling uses observed journey time data from equivalent time intervals on different dates of the same day type and link.

The accident data include geographical coordinates and exact time (rounded up to the minute level) of the accident occurrence. Using highly detailed GIS maps of the Ordnance Survey (OS VectorMap^{TM} District), I was able to identify the side of each two-way highway segment that each accident occurred. Using the level of detail of these two datasets, I have matched the information of the two datasets for the whole highway network of England. Map 4.3 in the Appendix shows the distribution of accidents in the highway network, as well as the metropolitan areas and the central cities of England.

Table 4.1: Descriptive Statistics

Variables	N	Mean	SD	Min	Max
2012					
Flow (vh/link/15-min)	37,543,968	432.4	371.9	0.12	2,888.5
Average speed	37,736,064	104.4	15.3	1.54	230.03
Average journey time (sec/link)	37,736,064	185.3	144.1	7.94	6,950.4
Journey time (sec/link km)	37,736,064	35.8	11.8	1.57	2,341.5
Accident	37,736,064	2,140			
Congested segments (<100km/h)	37,736,064	0.25			
Congested segments (<70km/h)	37,736,064	0.02			
Data quality (1-5)°	37,736,064	1.24			
Link length (km)	37,736,064	5.38	4.2	0.22	22.08
2013					
Flow (vh/link/15-min)	37,543,968	435.0	373.8	0.12	2,888.5
Average speed	37,736,064	103.3	15.6	1.71	557.29
Average journey time (sec/link)	37,736,064	187.5	148.5	5.85	9,938.0
Journey time (sec/link km)	37,736,064	36.3	12.2	6.46	2,104.2
Accident	37,736,064	2,040			
Congested segments (<100km/h)	37,736,064	0.28			
Congested segments (<70km/h)	37,736,064	0.02			
Data quality (1-5)°	37,736,064	1.16			
Link length (km)	37,736,064	0.54	0.4	0.21	22.08
2014					
Flow (vh/link/15-min)	37,543,968	440.4	375.9	0.25	2,888.5
Average speed	37,736,064	102.2	16.2	1.5	231.0
Average journey time (sec/link)	37,736,064	189.9	150.5	7.92	8,417.54
Journey time (sec/link km)	37,736,064	36.9	14.0	1.56	2,400
Accident	37,736,064	2,418			
Congested segments (<100km/h)	37,736,064	0.31			
Congested segments (<70km/h)	37,736,064	0.03			
Data quality (1-5)°	37,736,064	1.12			
Link length (km)	37,736,064	0.54	0.4	0.22	22.08

Notes: °Data quality is an indicator showing the quality of the journey time data for the link and time period. 1 indicates the highest quality data and 5 the lowest. *The accident number is reported instead of the mean.

Table 4.1 presents some descriptive statistics of the final dataset. Given the volume of the data and the fact that most estimates of Section 4.4 are presented for each year separately, I also present the descriptive statistics for each year separately. As it can be seen, the average flow is relatively constant throughout the whole period of study while the yearly standard deviation of the flow variable is relatively high. Average speed has a mean which approximates the standard level of free flow speed (100km/h) and a low standard deviation. Average journey times also exhibit a high standard deviation and a maximum value of approximately 46 minutes. The average journey time is roughly 3 minutes. Given that the average journey time depends on the length of each highway segment, I normalise this measure by highway km. I call the normalised variable journey time. The mean of journey time is about 36.3 sec/km while the maximum is about 38 minutes. I cannot observe a clear tendency of the number of accidents since their number declines until 2013 and in 2014 they increase substantially. The number of congested segments, defined as those where the average speed is below the free flow speed (100km/h)[9], appears to be increasing over time, highlighting the increasing severity of traffic congestion for the highway network in England. In addition, the average value of congested segments can be interpreted as follows. About 35 percent of the segments at all times are congested. This is considerably high since I also include the night time, where I expect no congestion in most of the network. Data quality is very high and is improving in the later years. Map 4.4 in the Appendix shows the location of the most congested segments. Not surprisingly, congestion bottlenecks are mainly formed near the highway exits to the main cities of England. Finally, the average link length is 5.6 km which ranges from very small (220m) to quite long (22km) links.

4.3 Methodology and Results

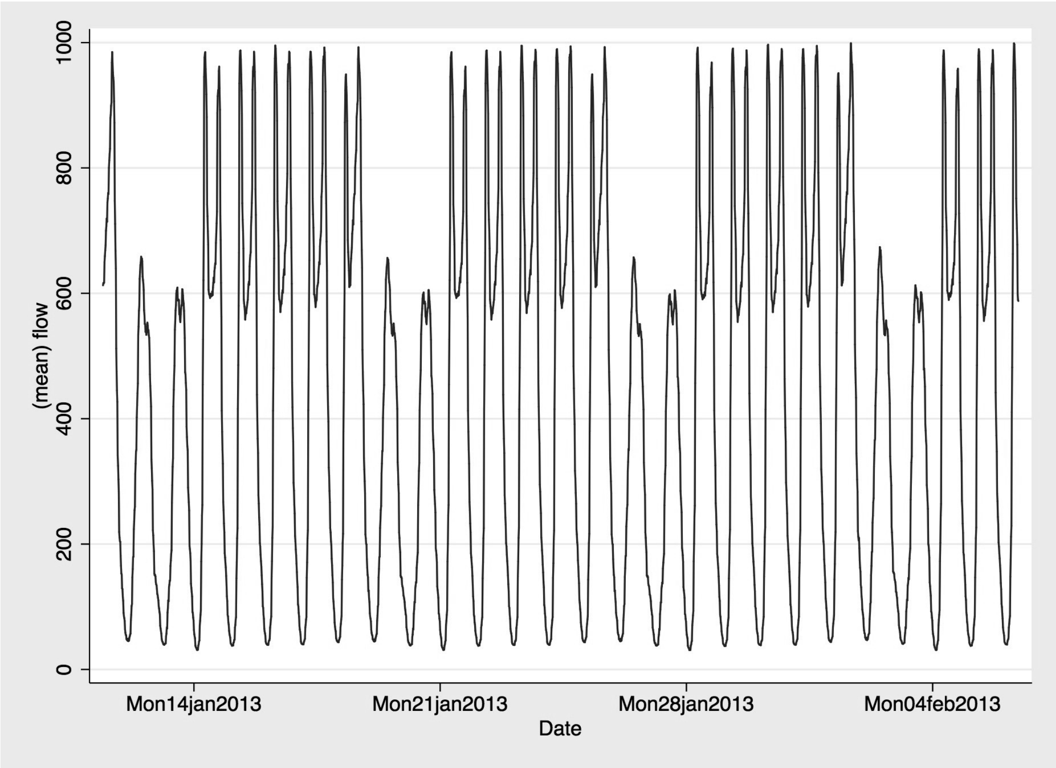
As in hourly electricity prices in day-ahead markets, traffic flows and average speeds exhibit specific characteristics such as mean-reversion, seasonality and spikes. However, in contrast with electricity markets, traffic flows do not have such a complex time-varying volatile structure. On the contrary, the stable weekly cycles of the traffic flows are those that will allow me to estimate the causal effect of highway accidents on traffic flows, average speeds and journey times. Figure 4.1 displays the average traffic flow for different times of the day for the Leeds area, as a representative example of the whole network. The traffic flow and average speed data exhibit a remarkably stable periodic pattern, which is repeated every week. These cycles of the traffic flow indicate that out of all the factors that may predict highway traffic, the time of the day and the day of the week are the two most important ones. Using the explanatory power of these two variables, I can define the recurrent traffic for a given time period which is virtually unchanged in the absence of any unexpected event.

This periodicity of traffic flows suggests that a forecasting model of traffic flows cannot treat time as "one-dimensional" (in a panel data meaning). Time-series mod-

[9] As it is often assumed in the literature. Almost all the highway segments included in the data have a speed limit of 70miles/h (112km/h).

els assume that the information set is updated by moving from one observation to the next in time. However, due to the nature of the road travel demand, I adopt the framework proposed by (Huisman et al., 2007), which, in this context, treats the 96 time periods of the day (of 15 minutes each) as 96 cross-sectional units that vary from day to day and in the different highway segments.

Figure 4.1: Example of flow periodicity.

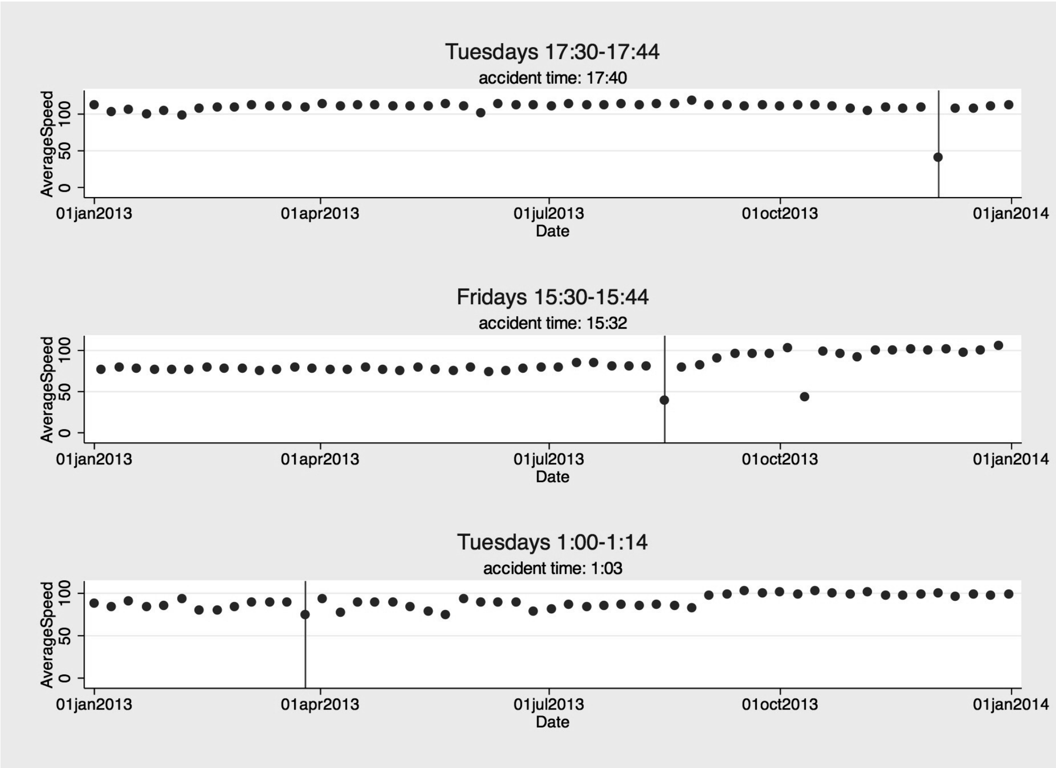


Source: Author's calculations based on average traffic flow data for Leeds area.

If an accident occurs, I expect that this stable day of the week and time-specific pattern of traffic flow will be disrupted. In figure 4.2, three examples of the average speed in different times of the day, during the same day of the week are depicted. As it can be seen, the average speed observed every week, on the same day of the week, at the same time is essentially the same for a whole year. In addition, it can be observed that the average speed drops significantly only during the day and the time that an accident happens (the vertical line). By being able to observe an almost perfect counterfactual of accident absence, the estimation of an accident incidence on traffic flow and average speeds will have a causal interpretation.

This stability of the average speed holds for almost all times of the day and days of the week. However, during night time and during weekends, this stability is more volatile. This can be explained by the nature of the demand for car travel. Car travel demand is highly inelastic before and after the standard "nine to five" working schedule during weekdays (mainly for commuting reasons). This makes the traffic flows (and consequently, average speeds and journey times) remarkably stable during these hours. The last graph of figure 4.2 shows an example of the average speed stability during night time (at 1 a.m.). Although the average speeds are less stable during this time, I can still observe a notable decrease of the average speed at the date of the accident compared to the other weeks.

Figure 4.2: Examples of average speed day of the week-time stability.



Notes: Based on average speed data at three different accident locations and times. The vertical line represents the time that an accident occurred.

Source: Authors own calculations based on the highway traffic data

Until this point, I have highlighted the persistence of traffic at each particular time of every day of the week. However, it should also be mentioned that as most time series processes, traffic flows and speeds at each time of the day also depend on the traffic of the preceding time period. Bottleneck models demonstrate the importance of such traffic flow dynamics (for more details, see Small and Verhoef (2007)). Figure 4.5 in the Appendix shows the variation in average speed using a continuous time dimension for the same accidents used in Figure 4.2. Again, I observe a substantial drop in average speed when an accident occurs.

4.3.1 Econometric framework: Big data approach

In this section, I describe the simple econometric framework that I use to estimate the effect of highway accidents on traffic congestion. In specifications (4.1) and (4.2) that follow, I use the journey time per highway km (*jt*) as the traffic variable of interest. While journey time is probably the best traffic measure to capture the effect on congestion, in Section 4.4, I also estimate the effect of an accident on traffic flows and average speeds, following specifications (4.1)-(4.5).

$$jt_{i,d,t} = \alpha_1 jt_{i,d,t-1} + \alpha_2 med(jt_{i,d+n*7,t}) + \alpha_3 \sum accident_{i,d,t} + \epsilon_{i,d,t} \tag{4.1}$$

where $jt_{i,d,t}$ is the average journey time in the highway segment *i*, on the date *d* and during the 15-minute period *t*. The lagged average journey time variables for the previous time period ($jt_{i,d,t-1}$) and the median journey time of the same day of the week during the same time period for four weeks before and four weeks after the date that the accident happened, ($med(jt_{i,d+n*7,t})$), is the variable that captures the recurrent congestion[10]. Based on the notation of specification (4.1), *n* is an integer which takes values in the interval $[-4,0) \cup (0,4]$. The dummy variable $accident_{i,d,t}$ takes the value 1 only when an accident occurs at the highway segment *i* on the date *d* and during the 15-minute period *t* and it is zero otherwise. This is the main variable of interest and its coefficient α_3 captures the marginal effect of the accident occurrence on journey time. I use the summation symbol before the accident dummy because I also use time lags of this variable in order to estimate the duration of this effect. Finally, $\epsilon_{i,d,t}$ is the error term which is highway segment, date and time-specific.

It is obvious that a naïve specification like specification (4.1) is susceptible to omitted variable bias, since the error term and the lagged average journey time are obviously correlated. In specification (4.2), I use first differences of the dependent variable and the median of the first differences as the variable that captures the recurrent congestion. In this setting, I control for unobservable variables that are time-invariant in the same highway segment for each specific date and for a period of 30 minutes. Such unobservable factors are road characteristics, the daily traffic patterns, holidays, while road condition and weather are also controlled to a large

[10] Similar to Adler et al. (2013).

extent[11]. Using specification (4.2), I am able to estimate the effect of an accident on highway congestion using very big datasets, with no need for additional control variables.

$$\Delta(jt_{i,d,t}) = \alpha_1 median(\Delta(jt_{i,d\pm n*7,t})) + \alpha_2 \sum accident_{i,d,t} + \epsilon_{i,d,t} \tag{4.2}$$

4.3.2 Econometric framework: Reduced sample approach

One could argue that the occurrence of an accident is a relatively rare event. As such and given the number of available counterfactuals (control group) in big datasets, a large part of this information might be redundant. In this section, I will use simple and double differences (or differences-in-differences), using a reduced sample of observations, in order to estimate the effect of an accident on traffic congestion. Using the simple difference approach, I only keep the observations where an accident occurred and the counterfactual observations that I previously used to calculate the median i.e. the average journey time at the same highway segment, on the same day of the week and at the same time of the day for four weeks before and four weeks after the accident. This sample comprises only 0.5 percent of the number of observations in the big dataset that we used in Section 4.4.1. By including segment-day of the week-time specific fixed effects to capture recurrent congestion, I estimate the effect of a highway accident on non-recurrent congestion. Equation (4.3) is the specification of these simple differences.

$$\Delta(jt_{i,d,t}) = \alpha_2 accident_{i,d,t} + \eta^{i,day\ of\ week,t} + \epsilon_{i,d,t} \tag{4.3}$$

For the diff-in-diff approach, my sample includes the observations of four time periods (one hour) before and after the accident occurrence for the day that the accident occurs, as well as for the same day of the week, four weeks before and after the date of the accident. However, in this case I will use the median first difference of the congestion variables as in specification (4.2) instead of the fixed effects I used in specification (4.3) because the number of fixed effects needed is too big for the matrix to be inverted. Specification (4.4) is the specification of these double differences, which also includes highway segment-date specific fixed effects. Using this approach, I can also estimate the duration of the effect as I did in specification (4.2) using a reduced sample of the data.

[11] Assuming that weather changes in 30-minute intervals are minor. I test this hypothesis by only including the accidents that were reported with good weather, on a dry road, with good lighting conditions and where no other special conditions were reported. My results in this case are very similar.

$$\Delta(jt_{i,d,t}) = \alpha_1 median(\Delta(jt_{i,d\pm n*7,t})) + \alpha_2 \sum accident_{i,d,t} + \eta^i * \eta^d + \epsilon_{i,d,t} \quad (4.4)$$

4.3.3 Econometric framework: Reverse relationship

As mentioned in the Introduction, the goal of this paper is to estimate the two-way relationship between accidents and congestion. In this section, I describe the identification strategy for the reverse relationship. The dependent variable of specification (4.5) is a dummy variable which takes the value one if an accident occurred in the highway segment *i*, on date *d* and in the 15-minutes time interval *t*. The main variable of interest is expressed in logarithms so that the estimated coefficient can be interpreted as a semi-elasticity. In order to take into account weather, I only include the accidents that were reported with good weather, on a dry road, with good lighting conditions and where no other special conditions were reported. In addition, I use highway segment-date specific fixed effects to control for any special events and other time invariant (in a two-hour and a quarter time interval) unobservable variables.

$$accident_{i,d,t} = \alpha_0 + \alpha_1 log(jt_{i,d,t-1}) + \eta^i * \eta^d + \eta^t + \epsilon_{i,d,t} \quad (4.5)$$

4.4 Main Results

4.4.1 Big data: Average results

Table 4.2 presents the results of specification (4.2) for all England in 2012, 2013 and 2014, using traffic flows, average speeds and journey times as alternative dependent variables. Columns [1], [2] and [3] report the estimated effect of an accident on traffic for 2012, 2013 and 2014, respectively. By including the lags of the accident dummy, I also obtain estimates of the dynamic effect. As can be seen in Columns [1]-[3], there is a small negative effect of an accident on traffic flows, which only lasts for the first 15 minute interval since the accident occurred. On the other hand, as it can be seen in Columns [4]-[6] and [7]-[9], the estimated effect of an accident on average speeds and journey times remains significant for an hour after the accident occurrence. Nevertheless, this effect is considerable only for the first 30 minutes after the accident, while the effect drops by 70-75 percent after the first 15 minutes. This result is in line with the findings of Adler et al. (2013) which suggests that accident duration has a negative but concave effect on non-recurrent congestion. This could be driven by the time needed for an accident to be completely removed from the highway. For the Netherlands, Snelder et al. (2013) report that the average removal time is 45 minutes.

The results of Table 4.2 suggest that the occurrence of an accident caused on average a reduction of 1 vehicle/link/15-min, which is arguably a very low effect compared to the mean flow (435 vh/link/15-min) for the highways of England. This minor effect of an accident on traffic flows can be explained in Figure 4.6. As the stock of vehicles in a segment increases, flow increases up to the point (D_m), where it starts decreasing (this situation is known as 'hypercongestion'). Therefore, an accident could increase or decrease the flow of vehicles depending on the initial level of vehicle density in the highway segment at the time of the accident. As a result, without taking into account the initial level of congestion in each highway segment, the positive and negative effects on traffic flow might counteract with each other. Thus, this minor negative effect on average traffic flows can be interpreted as the net effect of these opposing effects.

Table 4.2: All data

Dependent variable:	$\Delta(flow_{i,d,t})$			$\Delta(speed_{i,d,t})$			$\Delta(jt_{i,d,t})$		
	2012	2013	2014	2012	2013	2014	2012	2013	2014
	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]
$median(\Delta(traffic_{i,d+n*7,t}))^\dagger$	0.962^a	0.961^a	0.960^a	0.552^a	0.463^a	0.431^a	0.700^a	0.649^a	0.649^a
	(0.0002)	(0.0002)	(0.0002)	(0.0007)	(0.0008)	(0.0009)	(0.0021)	(0.0028)	(0.0030)
$accident_{i,d,t}$	-1.479^a	-1.149^b	-0.927^c	-7.918^a	-7.801^a	-7.684^a	6.401^a	6.418^a	6.350^a
	(0.446)	(0.488)	(0.499)	(0.353)	(0.370)	(0.308)	(0.472)	(0.511)	(0.414)
$accident_{i,d,t-1}$	0.404	0.451	-0.415	-2.569^a	-2.394^a	-2.122^a	1.629^a	1.579^a	1.573^a
	(0.454)	(0.510)	(0.525)	(0.223)	(0.249)	(0.184)	(0.259)	(0.302)	(0.223)
$accident_{i,d,t-2}$	0.0709	-0.166	0.446	-1.064^a	-1.085^a	-0.597^a	0.635^a	0.833^a	0.635^a
	(0.436)	(0.469)	(0.454)	(0.213)	(0.222)	(0.194)	(0.219)	(0.259)	(0.239)
$accident_{i,d,t-3}$	1.075^b	0.302	0.402	-0.601^a	-1.151^a	-1.058^a	0.521^a	1.055^a	0.591^a
	(0.447)	(0.490)	(0.442)	(0.195)	(0.199)	(0.180)	(0.199)	(0.207)	(0.189)
$accident_{i,d,t-4}$	0.340	-0.490	0.335	-0.434^b	-0.959^a	-0.842^a	0.499^b	1.122^a	0.545^a
	(0.429)	(0.446)	(0.446)	(0.193)	(0.215)	(0.176)	(0.196)	(0.227)	(0.179)
Observations (thousands)	37,153	36,878	36,994	37,343	37,033	37,033	37,343	37,033	37,033
R^2	0.884	0.874	0.866	0.057	0.033	0.028	0.044	0.046	0.047

Notes: Δ refers to first differences in time periods *t*. †The median is calculated $\forall n \in [-4, 0) \cup (0, 4]$. Robust standard errors clustered by highway segment and date are in parenthesis. ^{c}, ^{b} and ^{a} indicate significant at 1, 5, and 10 percent level, respectively.

On the other hand, the effect on average speeds and average journey times is considerably high. Specifically, a reduction of 7.8km/hour is a 7.5 percent reduction compared to the average speed while the increase in journey time is about 17.8 percent compared to the average journey time. This difference could be explained by the low average speeds that are observed during peak hours. At a time where the speeds are low, the effect of an accident on speeds is expected to be limited. However, a small decrease in the low speed could have an important effect in journey times when recurrent congestion is present.

If I sum the journey time delays for the four periods after the accident occurrence, I obtain a total delay of about 9.7 sec per highway km. This is an increase of about 27 percent compared to the mean journey time. Taking into account the average flow in a segment, the total time loss for each km due to the accident is approximately 70 minutes. This is an interesting back-of-the-envelope calculation to show us the importance of this effect.

4.4.2 Big data: Congested segments

Table 4.3 presents the results for the recurrently congested segments of the network in each year. I define congested segments as those where the mean speed averaged for each day of the week and time period of each month[12] is below 100km/h (free-flow speed).

Table 4.3: Monthly congested segments data (below 100km/h).

Dependent variable:	$\Delta(flow_{i,d,t})$			$\Delta(speed_{i,d,t})$			$\Delta(jt_{i,d,t})$		
	2012	2013	2014	2012	2013	2014	2012	2013	2014
	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]
$median(\Delta(traffic_{i,d+n*7,t}))^{\dagger}$	0.960^a	0.956^a	0.955^a	0.659^a	0.608^a	0.601^a	0.740^a	0.685^a	0.683^a
	(0.0004)	(0.0004)	(0.0004)	(0.0012)	(0.0013)	(0.0013)	(0.0028)	(0.0036)	(0.0036)
$accident_{i,d,t}$	-1.998^b	-0.856	-1.650	-9.187^a	-7.620^a	-8.774^a	10.60^a	9.066^a	10.70^a
	(0.998)	(1.111)	(1.046)	(0.707)	(0.685)	(0.547)	(1.241)	(1.178)	(0.920)
$accident_{i,d,t-1}$	0.696	-0.0675	-0.781	-3.482^a	-3.050^a	-2.763^a	2.862^a	2.881^a	3.198^a
	(1.164)	(1.173)	(1.087)	(0.466)	(0.499)	(0.345)	(0.616)	(0.856)	(0.577)
$accident_{i,d,t-2}$	-0.0537	-1.605	-0.321	-1.224^a	-2.332^a	-1.415^a	1.553^b	2.242^a	1.508^b
	(1.119)	(1.073)	(0.972)	(0.460)	(0.460)	(0.369)	(0.659)	(0.670)	(0.617)
$accident_{i,d,t-3}$	1.505	0.896	-0.786	-1.454^a	-2.117^a	-1.472^a	1.667^a	2.253^a	1.185^b
	(1.086)	(1.093)	(0.960)	(0.421)	(0.426)	(0.359)	(0.645)	(0.560)	(0.499)
$accident_{i,d,t-4}$	-1.538	-1.157	-0.940	-0.774^c	-1.433^a	-1.316^a	0.904	2.843^a	1.199^b
	(1.118)	(1.043)	(1.010)	(0.433)	(0.474)	(0.326)	(0.631)	(0.718)	(0.502)
Observations (thousands)	9,229	10,309	11,395	9,279	10,344	11,398	9,383	10,344	11,398
R^2	0.863	0.848	0.841	0.101	0.074	0.070	0.053	0.057	0.057

Notes: Δ refers to first differences in time periods *t*. †The median is calculated $\forall n \in [-4,0) \cup (0,4]$. Robust standard errors are clustered by highway segment and date and are in parenthesis. ^{c}, ^{b} and ^{a} indicate significant at 1, 5, and 10 percent level, respectively.

[12] I use the average for each month based on the assumption that in each month, the pattern of flows is stable. This assumption is verified by the data.

In Table 4.3, the results for flow are similar to the previous results in Table 4.2. However, there is no significant effect for 2013 and 2014. Again, since I define congested segments based on a high speed threshold (100km/h), I may be capturing counteracting effects on flows. On the other hand, the average speed dropped about 9.7 percent compared to the average speed in congested segments. In addition, an accident increases journey time by 22.7 percent compared to the average journey time. These coefficients are higher, compared to the results in Table 4.2, showing that the delays caused by an accident in times of recurrent congestion are the major issue.

Table 4.4 uses an alternative, more conservative speed threshold for the definition of a congested segment. Instead of using the common threshold of the free flow speed (100km/h), I assume a 70km/h threshold. The first three columns of Table 4.4 show that when highly congested segments are considered, the effect of an accident on flows is in most cases negative and higher than the estimated effect in Tables 4.2 and 4.3, albeit not statistically significant[13]. For these heavily congested segments, the effect of an accident on average speeds and journey times is roughly the same as in Table 4.3 (14.7 and 21.6 percent, respectively, compared to the average).

Table 4.4: Monthly congested segments data (below 70km/h).

Dependent variable:	$\Delta(flow_{i,d,t})$			$\Delta(speed_{i,d,t})$			$\Delta(jt_{i,d,t})$		
	2012	2013	2014	2012	2013	2014	2012	2013	2014
	OLS [1]	OLS [2]	OLS [3]	OLS [4]	OLS [5]	OLS [6]	OLS [7]	OLS [8]	OLS [9]
$median(\Delta(traffic_{i,d+n*7,t}))^{\dagger}$	0.957^a	0.944^a	0.942^a	0.836^a	0.821^a	0.811^a	0.784^a	0.724^a	0.710^a
	(0.0014)	(0.0016)	(0.0013)	(0.0025)	(0.0025)	(0.0024)	(0.0040)	(0.0049)	(0.0046)
$accident_{i,d,t}$	-5.484^c	0.369	-2.709	-8.171^a	-6.433^a	-10.19^a	14.96^a	12.47^a	21.18^a
	(3.051)	(3.916)	(2.854)	(1.367)	(1.420)	(1.070)	(3.554)	(3.619)	(2.723)
$accident_{i,d,t-1}$	-3.748	0.912	-2.077	-3.465^a	-4.278^a	-4.218^a	7.518^a	5.657^b	6.828^a
	(4.281)	(3.497)	(3.264)	(1.044)	(1.231)	(0.842)	(2.659)	(2.551)	(1.599)
$accident_{i,d,t-2}$	0.635	-0.156	0.257	0.0958	-3.179^a	0.116	0.328	2.368	-0.674
	(3.491)	(3.140)	(3.153)	(1.104)	(1.141)	(0.857)	(2.823)	(2.162)	(2.274)
$accident_{i,d,t-3}$	0.692	-4.587	-1.079	-1.426	-2.255^b	-1.698^b	4.090	3.869^b	2.175
	(4.129)	(3.574)	(3.263)	(1.122)	(1.014)	(0.759)	(2.508)	(1.873)	(1.583)
$accident_{i,d,t-4}$	-9.117^b	3.067	-5.257	0.814	-3.761^a	-2.289^b	0.832	9.770^a	2.811
	(4.201)	(3.512)	(3.467)	(1.163)	(1.190)	(1.068)	(2.117)	(2.523)	(1.906)
Observations	741,195	824,049	978,802	745,248	825,042	979,113	760,754	824,572	979,113
R^2	0.758	0.712	0.706	0.290	0.252	0.233	0.094	0.086	0.081

Notes: Δ refers to first differences in time periods *t*. †The median is calculated $\forall n \in [-4, 0) \cup (0, 4]$. Robust standard errors are clustered by highway segment and date and are in parenthesis. ^{c}, ^{b} and ^{a} indicate significant at 1, 5, and 10 percent level, respectively.

[13] Only marginally statistically significant in 2012.

4.4.3 Big Data: Rubbernecking

'Rubbernecking' refers to drivers trying to view the aftermath of a traffic accident. The term refers to the physical act of craning one's neck, performed in order to get a better view. Table 4.5 presents the results when I regress the traffic variables on an accident that happened on the opposite direction of the highway segment. The index *j* refers to the opposite direction of the highway segment that an accident took place. While I expected to find some negative effect based on the hypothesis that people reduce their speed in order to satisfy their curiosity, for the average journey time in 2013 and 2014, I find a small effect with the opposite sign. Only for traffic flows, the coefficient for rubbernecking has the expected sign. One possible explanation for the opposite effect on average speed and journey times is because of traffic deviation due to an accident. Modern GPS and mobile applications inform the users about the occurrence of an accident instantaneously. Subsequently, many users choose to deviate from that route[14]. However, the information about the accident is not always direction-specific. Therefore, the users on the opposite direction of the highway segment that the accident took place might actually experience reduced congestion and thus, higher speeds and lower journey times. This explanation is corroborated by the fact that traffic flows decreased significantly in 2012 and 2014.

Table 4.5: All England: Rubberneck congestion.

Dependent variable:	$\Delta(flow_{j,d,t})$			$\Delta(speed_{j,d,t})$			$\Delta(jt_{i,d,t})$		
	OLS 2012	OLS 2013	OLS 2014	OLS 2012	OLS 2013	OLS 2014	OLS 2012	OLS 2013	OLS 2014
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]
$median(\Delta(traffic_{j,d+n*7,t}))^{\dagger}$	0.962^a	0.961^a	0.960^a	0.552^a	0.463^a	0.431^a	0.700^a	0.649^a	0.649^a
	(0.0002)	(0.0002)	(0.0002)	(0.0007)	(0.0008)	(0.0009)	(0.0021)	(0.0028)	(0.0030)
$accident_{i,d,t}$	-1.375^c	-0.162	-1.782^a	0.082	0.627	0.957^a	0.013	-0.360^b	-0.377^a
	(0.746)	(0.960)	(0.577)	(0.429)	(0.387)	(0.241)	(0.285)	(0.176)	(0.132)
$accident_{i,d,t-1}$	0.939	-0.666	0.211	0.744	-0.252	-0.775^a	-0.502	0.191	0.294^c
	(1.214)	(1.381)	(0.602)	(0.660)	(0.541)	(0.299)	(0.463)	(0.278)	(0.152)
$accident_{i,d,t-2}$	0.443	1.644	1.460^b	-0.490	0.388	0.142	0.459	-0.308	-0.006
	(1.385)	(2.166)	(0.607)	(0.764)	(0.625)	(0.280)	(0.492)	(0.404)	(0.142)
Observations (thousands)	37,153	36,878	36,994	37,343	37,033	37,033	37,343	37,033	37,033
R^2	0.884	0.874	0.866	0.056	0.033	0.028	0.044	0.046	0.047

Notes: Index *j* refers to the opposite highway segment from the link that the accident took place. Δ refers to first differences in time periods *t*. †The median is calculated $\forall n \in [-4, 0) \cup (0, 4]$. Robust standard errors clustered by highway segment and date are in parenthesis. ^{c}, ^{b} and ^{a} indicate significant at 1, 5, and 10 percent level, respectively.

[14] This implies that all my estimates for the effect of an accident on traffic congestion might underestimate the real effect if traffic deviation was also considered.

4.4.4 Very reduced sample: Simple differences

Until this point, I have used the bulk of information in this big traffic dataset in order to analyse the effect of an accident on traffic congestion. In this section, I use a very reduced sample of the traffic data in order to estimate the same effect. In order to do this, I include the observations where an accident happened as the treatment group and the same day of the week at the same time for four weeks before and four weeks after the date that the accident happened as the control group. These are the observations that I used in Sections 4.4.1 to construct the median that captures the recurrent congestion (for details, see Section 4.3.1). In order to control for unobservable variables that are invariant in the same highway segment, in each specific day, for a period of 30 minutes, I use again first differences of the dependent variable as I did in Section 4.4.1. In addition, I use day of the week-time specific fixed effects in order to capture the recurrent congestion in an alternative way, as in specification (4.3). The results are presented in Table 4.6. The results are very similar with the previous results in Section 4.4.1, suggesting that an accident causes on average a 7.9km/h reduction in average speeds and an increase in journey times of 6.5sec/km in the same 15-minute interval that the accident happened.

Table 4.6: Simple differences.

Dependent variable:	$\Delta(flow_{i,d,t})$			$\Delta(speed_{i,d,t})$			$\Delta(jt_{i,d,t})$		
	2012	2013	2014	2012	2013	2014	2012	2013	2014
	OLS [1]	OLS [2]	OLS [3]	OLS [4]	OLS [5]	OLS [6]	OLS [7]	OLS [8]	OLS [9]
$accident_{i,d,t}$	-1.631^{a}	-1.216^{b}	-0.938^{c}	-7.912^{a}	-7.963^{a}	-7.793^{a}	6.279^{a}	6.438^{a}	6.365^{a}
	(0.481)	(0.510)	(0.533)	(0.348)	(0.353)	(0.305)	(0.456)	(0.477)	(0.414)
Highw. segment-day of the week-time FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Observations	17,807	18,363	19,766	17,931	18,535	19,829	17,931	18,535	19,829
R^2	0.843	0.832	0.803	0.208	0.212	0.232	0.185	0.180	0.196

Notes: The number of observations in each year differs because of the different number of accidents in each year. Δ refers to first differences in time periods t. †The median is calculated $\forall n \in [-4, 0) \cup (0, 4]$. Robust standard errors clustered by highway segment, day of the week and time are in parenthesis. ^{c}, ^{b} and ^{a} indicate significant at 1, 5, and 10 percent level, respectively.

4.4.5 Very reduced sample: Double differences

In the Section 4.4.4, I only estimated the instantaneous effect of an accident on congestion using a very reduced sample. By 'instantaneous', I mean that I only considered the effect of an accident, which happened in the 15-minute interval that the traffic speeds and journey times were measured, on the same 15-minutes interval. In this section, I include lags of the accident dummy, which measure the effect of an accident in the 15-minute periods following the 15-minute interval during which an accident happened, as in Section 4.4.1. This approach is essentially a differences-in-differences estimation. The results presented in Table 4.7 follow specification (4.4).

Table 4.7: Differences-in-differences

Dependent variable:	$\Delta(flow_{i,d,t})$			$\Delta(speed_{i,d,t})$			$\Delta(jt_{i,d,t})$		
	2012	2013	2014	2012	2013	2014	2012	2013	2014
	OLS [1]	OLS [2]	OLS [3]	OLS [4]	OLS [5]	OLS [6]	OLS [7]	OLS [8]	OLS [9]
$median(\Delta(traffic_{i,d+n*7,t}))^\dagger$	0.961^a	0.933^a	0.939^a	0.277^a	0.265^a	0.315^a	0.566^a	0.493^a	0.590^a
	(0.004)	(0.005)	(0.005)	(0.013)	(0.013)	(0.012)	(0.045)	(0.030)	(0.035)
$accident_{i,d,t}$	-1.168^b	-0.580	-0.215	-8.402^a	-8.409^a	-7.931^a	5.622^a	5.995^a	5.284^a
	(0.486)	(0.544)	(0.555)	(0.380)	(0.397)	(0.333)	(0.531)	(0.578)	(0.482)
$accident_{i,d,t-1}$	0.727	1.067^b	0.286	-3.024^a	-3.013^a	-2.320^a	0.815^b	1.150^a	0.469
	(0.504)	(0.533)	(0.550)	(0.267)	(0.290)	(0.226)	(0.359)	(0.401)	(0.330)
$accident_{i,d,t-2}$	0.399	0.438	1.131^b	-1.503^a	-1.639^a	-0.748^a	-0.203	0.347	-0.512
	(0.472)	(0.518)	(0.505)	(0.253)	(0.263)	(0.231)	(0.316)	(0.364)	(0.348)
$accident_{i,d,t-3}$	1.442^a	0.908^c	1.057^b	-1.009^a	-1.749^a	-1.202^a	-0.349	0.587^c	-0.567^c
	(0.505)	(0.526)	(0.501)	(0.231)	(0.250)	(0.224)	(0.308)	(0.340)	(0.306)
$accident_{i,d,t-4}$	0.678	0.030	0.989^c	-0.856^a	-1.489^a	-0.979^a	-0.381	0.607^c	-0.625^b
	(0.483)	(0.505)	(0.511)	(0.234)	(0.258)	(0.222)	(0.310)	(0.335)	(0.300)
Highw. segment-date FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Observations	165,849	160,860	185,482	167,028	162,390	186,094	167,028	162,390	186,094
R^2	0.803	0.755	0.757	0.025	0.024	0.031	0.022	0.023	0.031

Notes: The number of observations in each year differs because of the different number of accidents in each year. Δ refers to first differences in time periods *t*. †The median is calculated $\forall n \in [-4, 0) \cup (0, 4]$. Robust standard errors clustered by highway segment and date are in parenthesis. ^{c}, ^{b} and ^{a} indicate significant at 1, 5, and 10 percent level, respectively.

Table 4.7 confirms the results of Table 4.2. The results are again very similar using a very reduced sample. Only the small negative effect on traffic flows is not statistically significant in this context. That could be explained by the higher standard errors in a regression with about a thousandfold less observations.

4.4.6 Reverse relationship: Accident by congestion

In this section, I present the results of estimating the reverse relationship between congestion and accidents. The relationship between congestion and accidents is expected to be non-linear, as suggested in the literature (Christensen and Amundsen, 2005; Lord et al., 2005). I use a cubic relationship, which I find that best fits the data. However, because of the high correlation between the log variables and their square and cubic terms, I subtract the mean of each variable and then took the logarithms.

The sample that I use in this analysis is the same as the one in Section 4.4.5 However, in Table 4.8, I estimate the effect for the whole period 2012-2014. In addition, I only include the accidents that were reported with good weather, on a dry road, with good lighting conditions and where no other special conditions were reported to minimise omitted variable concerns because of weather conditions. Moreover, I use highway segment-date specific fixed effects to control for any special events and other time invariant unobservable variables (in a two-hour and a quarter time interval). Following specification (4.5), the dependent variable is a dummy variable which takes the value one when an accident occurs. Table 4.8 presents a non-linear OLS regression for the log of the demeaned flow, average speed and journey time using a Linear Probability Model (LPM)[15].

Table 4.8: Reverse relationship: logs

Dependent variable:	$accident_{i,d,t}$				
	OLS [1]		OLS [2]		OLS [3]
$ln(flow_{i,d,t-1} - \overline{flow_{i,d,t}})$	-0.0008	$ln(speed_{i,d,t-1} - \overline{speed_{i,d,t}})$	0.0080^{a}	$ln(jt_{i,d,t-1} - \overline{jt_{i,d,t}})$	-0.0158^{a}
	(0.0006)		(0.0018)		(0.0017)
$(ln(flow_{i,d,t-1} - \overline{flow_{i,d,t}}))^2$	-0.0225^{a}	$(ln(speed_{i,d,t-1} - \overline{speed_{i,d,t}}))^2$	-0.0572^{a}	$(ln(jt_{i,d,t-1} - \overline{jt_{i,d,t}}))^2$	-0.0043
	(0.0019)		(0.0055)		(0.0028)
$(ln(flow_{i,d,t-1} - \overline{flow_{i,d,t}}))^3$	-0.0077^{a}	$(ln(speed_{i,d,t-1} - \overline{speed_{i,d,t}}))^3$	-0.0199^{a}	$(ln(jt_{i,d,t-1} - \overline{jt_{i,d,t}}))^3$	-0.0096^{a}
	(0.0010)		(0.0032)		(0.0028)
Highw. segment-date FE	✓		✓		✓
Timeperiod FE	✓		✓		✓
Observations	649,220	Observations	653,465	Observations	653,471
R^2	0.001	R^2	0.002	R^2	0.001

Notes: The log variables for flow, average speed and journey times are demeaned i.e. from each variable I subtracted its mean value. Robust standard errors clustered by highway segment and date are in parenthesis. ^{c}, ^{b} and ^{a} indicate significant at 1, 5, and 10 percent level, respectively.

Column [1], [2] and [3] in Table 4.8 present the results of regressing the accident dummy on the log of the demeaned average flow, speed and journey time, respectively. Column [1] suggests no effect of traffic flows on the probability of an accident. When I use the average speeds or journey times as the main regressor in Columns [2] and [3], respectively, the results suggest that traffic congestion affects negatively the probability of an accident. Column [2] suggests that an increase in

[15] I also tried a dynamic panel IV approach as the one suggested by Anderson and Hsiao (1981) and extended later by Arellano and Bond (1991). However, instead of using the lags of the dependent variable or the lags of first differences as instruments, I used the median of the dependent variable for four weeks before and four weeks after the accident occurrence. By using such long instrument, one avoids issues related to instrument exogeneity while the stability of weekly traffic patterns ensures the relevance of this instrument. Although such an approach could be considered as a methodological novelty, the instruments are not strong enough to be used in a non-linear regression.

average speeds causes an increase in the probability of an accident. Specifically, a 10% increase in average speeds is associated with a 0.08 percent increase of the accident rate, which on average is 0.94 percent in our sample. In other words, a 10 percent increase in average speeds is associated with an increase of 8 percent, which is substantial. Turning to the results of Column [3], a 10 percent increase in journey time is associated with a decrease in the probability of an accident of 0.158 percent or in other words, a 16 percent of the average accident rate.

While these results suggest that highway congestion reduces the probability of an accident, it should be kept in mind that the data on accidents used in this analysis only include personal injury accidents. However, traffic congestion is negatively correlated to accident severity because of the low speeds (Shefer and Rietveld, 1997). Therefore, our results cannot be generalised for cases of very high recurrent congestion or hypercongestion.

4.5 Conclusions

In this paper, I present empirical evidence showing that highway accidents had a significant effect on highway congestion in England during the period 2012-2014. While I only find a minor negative effect on traffic flows, the marginal decrease of the average speed due to an accident is about 7.8km/h while journey time increases by roughly 27 percent when I consider the duration of this effect. Another important finding is that the effect decays by 70-75 percent after the first quarter of an hour. Such evidence suggests that accident removal services are quite efficient in England. Furthermore, the effect of an accident on non-recurrent congestion is more salient in the recurrently congested parts of the network.

'Rubbernecking' (i.e. drivers trying to view the aftermath of a traffic accident in the other direction that the accident happened) does not have any impact on highway congestion in England. Instead, I find a negative effect on traffic congestion in the other direction of the highway that the accident happened. This finding can be explained by the fact that accident reports and other navigation software often do not have real-time information about the direction that an accident occurred and thus, they relieve the congestion on the opposite direction.

I also use simple differences and differences-in-differences estimations using a very reduced sample of the big dataset. This exercise confirms the previous results and suggests that refining the meaningful information is the real challenge of 'big data'.

Regarding the effect of traffic congestion on the probability of an accident, I find no evidence of a positive effect. On the contrary, I find evidence of a non-linear convex negative effect, i.e. more congestion is associated with a decrease in the probability of an accident.

Finally, the ultimate goal of this paper is to conclude with a back-of-the-envelope calculation of the estimated effect of an additional accident on traffic congestion. It seems that on average an accident causes 70 minutes of traffic delay per km for the users of that particular highway segment, while this effect is 160 minutes in the recurrently congested segments. Therefore, for an average highway segment of about 5km, the total delay would be about 6 hours on average and about 14 hours for the congested segments. These figures can easily be converted to monetary terms and together with the benefit of decreasing the number of accidents by one, they can be used to determine a marginal cost threshold for policies that aim to reduce the number of accidents. Finally, the findings of this paper suggest that traffic management authorities would benefit from primarily focusing their efforts regarding accident prevention and accident removal, on the recurrently congested parts of the network.

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4.6 Appendix

Figure 4.3: Highway network, accidents and urban areas.

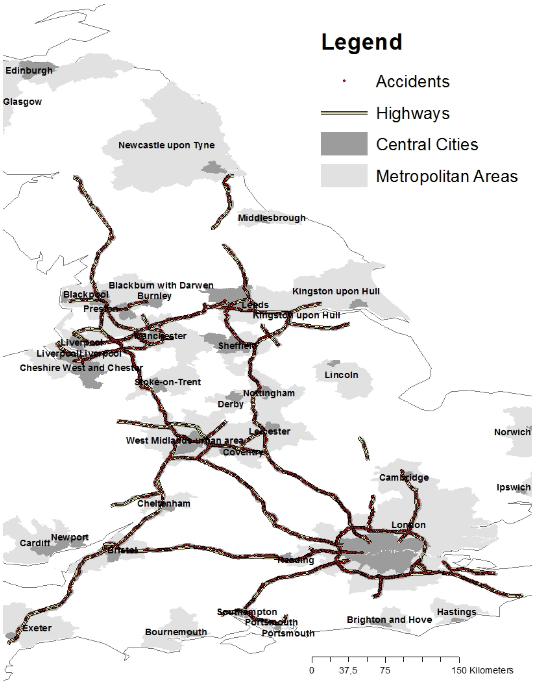


Figure 4.4: Congested highway segments and urban areas.

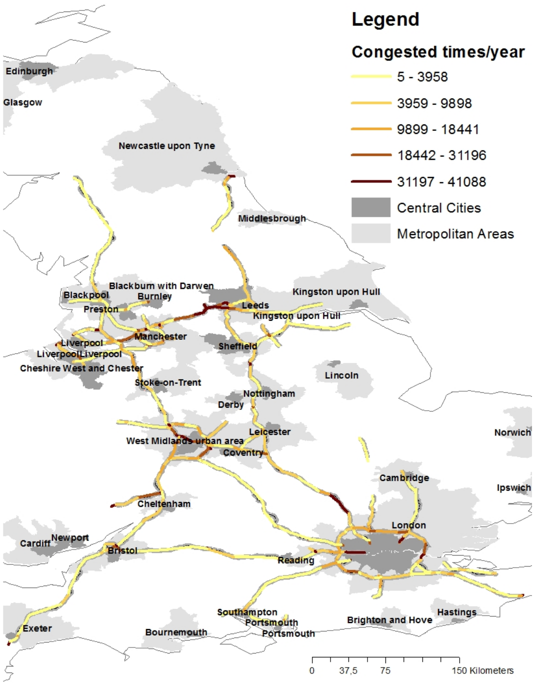
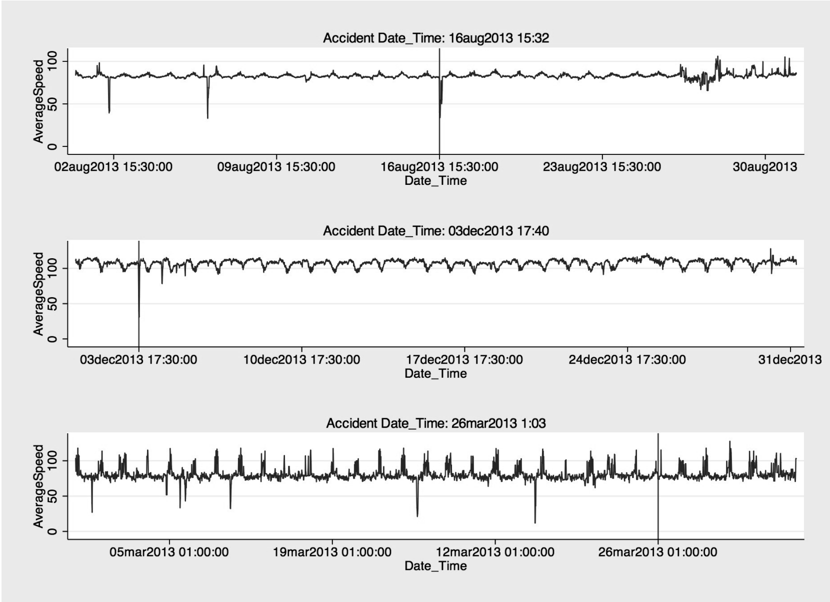
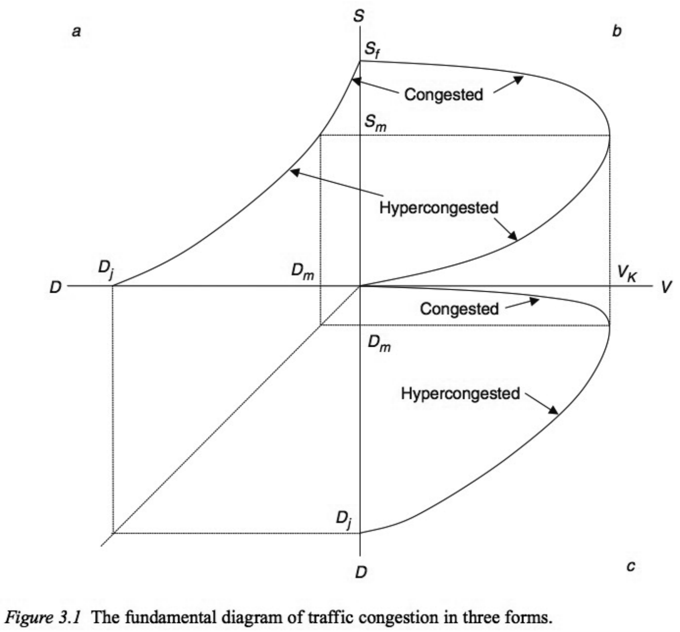


Figure 4.5: Examples of average speed variation over continuous time.



Notes: Based on average speed data at three different accident locations and times (same as in Figure 4.2). The vertical line represents the time that an accident occurred.

Figure 4.6: Speed-flow relationship: $V \equiv SD$



Source: Small and Verhoef (2007)

5 Shopping externalities and retail concentration: Evidence from Dutch shopping streets[§]

5.1 Introduction

One of the main reasons that people choose to live in a city is the presence of a rich variety of consumer goods and services offered by the retail sector (Glaeser et al., 2001). Shops tend to be concentrated in shopping streets and shopping districts, often located in city centres, or in shopping malls near the urban fringe. In European city centres, shops are mostly concentrated in pedestrianised shopping streets. As an illustration, walking is so important for shopping that the majority of all Dutch pedestrian movements occur while shopping[1].

Arguably, the most important reason for shops to cluster is the presence of *shopping externalities*, which are generated by consumers' 'trip-chaining' behaviour. Shopping externalities have a simple logic. In retail markets, transportation costs are usually paid by customers and incurred on a shopping trip basis (Claycombe, 1991). Consumers who visit several shops benefit from reductions in transport and search costs. In the context of shopping streets, a shop's productivity function depends on local footfall, which captures the number of pedestrians that pass a shop. Footfall tends to be higher in areas with more shops, since pedestrians tend to browse through shops in order to find the best shopping options. Hence, the associated reductions in costs for consumers imply a shopping externality *for shops*, which is enhanced when multiple shops are located in close proximity (Eaton and Lipsey, 1982; Claycombe, 1991; Schulz and Stahl, 1996)[2]. Similar to other agglomeration

[§]The paper in this chapter is coauthored with Hans R.A. Koster and Jos van Ommeren.

[1]This is based on data from Statistics Netherlands. We exclude hiking and recreational walking activities.

[2]Externalities arise when a sufficient number of pedestrians are involved in multipurpose shopping trips. If there is substantial heterogeneity between shops in generating footfall, the number of shops is a poor proxy for externalities. For instance, a popular clothing store is likely to generate substantial footfall, whereas a fast food store may not generate much footfall, but will benefit from footfall created by other shops.

advantages, these shopping externalities are expected to capitalise into store owners' rental income, defined as the shop rent paid by store owners multiplied by the share of the time that the shop is occupied[3]. In the current literature on retail location choices, there is a tendency to mainly focus on the issue of spatial competition and spatial or product differentiation (D'Aspremont et al., 1979; Osborne and Pitchik, 1987). Davis (2006) focuses on movie theatres, and evaluates consumers' transport costs, the effect of geographic differentiation, and the extent of market power among other things. Seim (2006) shows that there are significant returns to product (or spatial) differentiation and illustrates that markets with more scope for differentiation support greater entry. Jia (2008) and Arcidiacono et al. (2016) study the impact of Wal-Mart on the retail market, among others on incumbent (discount) supermarkets and small grocery stores. Zhou (2014) shows that multiproduct search, which is important when consumers buy multiple products in one shopping trip, can significantly influence retail firms' pricing decisions. Johansen and Nilssen (2016) investigate the conditions under which one-stop shopping causes the formation of big stores.

In the empirical literature, only limited attention has been given to the importance of shopping externalities. We are not the first to argue that the most important reason for shops to cluster is the presence of shopping externalities. However, this is the first paper that quantifies these externalities. We contribute to the literature in the following ways.

First, we introduce a unique measure of shopping externalities, *footfall*[4]. We argue and demonstrate that footfall is a superior measure of shopping externalities compared to the number of shops in the vicinity of a shop. The number of shops is an alternative measure which will underestimate the presence of shopping externalities when shops vary in the amount of footfall they generate[5]. We provide a number of arguments why footfall captures shopping externalities, and not simply captures local variation in shopping demand (e.g., we measure footfall on Saturdays, when pedestrians mainly walk for shopping; it predominantly includes shoppers who visit several shops). In contrast to the extensive retail literature which focuses on US shopping malls, we focus on the *full* population of main *shopping*

[3] In non-retail markets, agglomeration advantages also capitalise into wages (e.g., Arzaghi and Henderson (2008)). In retail markets, agglomeration economies occur very locally, so capitalisation into wages must be negligible, because differences in commuting time between competing shops within the same shopping district are negligible.

[4] In the retail industry, footfall is a standard measure to explain the attractiveness of a shopping location.

[5] Note that footfall and number of shops should have roughly the same effect when the amount of footfall generated per shop is the same for all shops. We will demonstrate that the elasticity of rental income with respect to footfall is more than doubled compared to the same elasticity with respect to number of shops.

streets of the Netherlands. In the Netherlands, as in the rest of Europe, shopping streets are much more common than shopping malls[6].

An important feature of shopping streets is that they are dominated by two sectors: clothing and cafés/restaurants. The main strategy followed by the shops in these sectors is to differentiate themselves by supplying heterogeneous products. This is in sharp contrast to other retail sectors that are examined in the economic literature, which offer homogeneous products and where spatial differentiation is the main strategy (e.g. movie theatres, gas stations, or video retailers, see Davis (2006); Netz and Taylor (2002); Seim (2006)).

It is important to note that shopping streets are characterised by a very different form of retail organization than shopping malls. In contrast to the evidence for shopping malls, we will show that property ownership in shopping streets is very fragmented. As a consequence, internalisation of shopping externalities does not occur in shopping streets[7]. Thus, policies that foster retail concentration by providing subsidies are potentially welfare improving[8]. We then make a distinction between subsidies given to (new) *store owners*, for which the level of generated footfall is unknown, and subsidies to specific *retail firms* for which is known how much footfall they generate. The former type of subsidy will stimulate more stores, whereas the second type of subsidy will stimulate the presence of footfall-generating retail shops.

The second, and main, contribution of the current paper is the identification of shopping externalities by estimating the causal effect of footfall on the rental income of store owners, which depends on the rent paid by tenants as well as the probability that a property lies empty. As has been widely discussed in the agglomeration literature, proxies for spatial concentration, such as footfall, tend to be endogenous because they are correlated to unobserved location characteristics. We address this issue by focusing on shops that are very close to each other (within 50m) but on different *intersecting* streets, controlling for an extensive set of shop and street characteristics[9]. Using spatial variation in footfall between intersecting

[6]Shopping centre floor space per person is more than tenfold in the US compared to Europe (2,150m^2 per thousand people in the US compared to 182m^2 per thousand people in Europe in 2011 (Cushman & Cushman, 2011).

[7]In shopping malls, property owners set the rent based on shop turnover, so shopping externalities are internalised. Thus, they charge lower rents to footfall-generating shops (or 'anchor stores') (Brueckner, 1993; Pashigian and Gould, 1998; Konishi and Sandfort, 2003), which could be regarded as a first-best subsidy.

[8]Many examples of such policies can be given. For many European countries, in particular Germany, it could be argued that pedestrianised areas subsidise local store owners, as the advantages are local whereas the disadvantages of prohibiting car use fall on other agents. Subsidies to park-and-ride facilities, including free public transport towards city centres is another similar example.

[9]Because people follow certain routes for their shopping trips, footfall strongly differs between intersecting streets. On average, the high-footfall street is roughly twice as 'busy' as its intersecting

streets, we control for unobserved locational endowments that attract both shoppers and shops (e.g. free parking). Our identifying assumption is that shops with similar preferences for location characteristics will locate in close proximity from each other while they will sort themselves into lower and higher footfall streets, depending on their preferences for footfall. Shops that benefit strongly from footfall (e.g. mainstream clothing shops) will sort into high-footfall streets and pay higher rents.

We show that footfall has a strong positive effect on rental income with an elasticity of approximately 0.25, whereas the elasticity of rental income with respect to the number of shops is 0.10. Thus, there are substantial external benefits from fostering footfall and retail concentration. Based on these estimates, the optimal subsidy that should be given to store owners amounts to about 10 percent of the rent, on average. However, for retail firms that generate a considerable amount of footfall for surrounding shops, this subsidy should be substantially higher.

The implications of our findings contribute to a heated policy debate on the decline of city centres in some European countries and the rise of large 'big-box' stores near the urban fringe (Sanchez-Vidal, 2016). It is also complementary to a literature which demonstrates that the welfare effects of current planning policies that *hinder* entry in retail markets, and particularly of large retailers, are negative. Several studies have shown that regulation policies reduce retail productivity and job growth and increase market power of incumbent stores (Bertrand and Kramarz, 2002; Schivardi and Viviano, 2011; Haskel and Sadun, 2012; Cheshire et al., 2015).

We subject our results to a wide range of robustness checks and ancillary regressions, for example by exploiting temporal rather than spatial variation in footfall, by investigating any potential negative external effects on house prices and by investigating differential effects of footfall on 'anchor' or chain-stores.

This paper continues as follows. In Section 5.2 we discuss the theoretical framework that guides the empirical results. Section 5.3 introduces the econometric framework, the data and reports the descriptive statistics. In Section 5.4, we present and discuss our results. Section 5.5 presents the counterfactual analysis to estimate rental income and determine the optimal subsidy. Section 5.6 summarises the sensitivity analysis, which is described in more detail in the Appendix (Section 5.8). We conclude in Section 5.7 and we discuss the policy implications of this study. In the Appendix, Sections 5.8.1 and 5.8.2 include the proofs for the prepositions made in Section 5.2, Section 5.8.3 is a Data Appendix, while Sections 5.8.4-5.8.10 describe in detail our sensitivity analysis.

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low-footfall street.

5.2 Theoretical framework

5.2.1 Rental income, rents and vacancies

We aim to measure the presence of shopping externalities by estimating the effect of footfall on (expected) rental income of store owners, denoted by I[10]. We allow for vacancies in the property market with a certain probability. The owners of vacant properties need advertising services to find a new tenant, which is costly. Given rent p and vacancy rate v, rental income of a property is given by:

$$I = p(1-v) - cv \tag{5.1}$$

where $p(1-v)$ is rental income when the property is let out to a tenant and cv is the advertising costs. It seems reasonable to assume that, at least in the long run, the advertising costs c are proportional to p, so $c = \kappa p$, where $\kappa > 0$. Because vacancy rates tend to be small, $log(1-(1+\kappa)v) \approx -(1+\kappa)v$. Hence, the *logarithm* of rental income, $logI$, is then (approximately) equal to $logp - (1+\kappa)v$.

Let us now suppose that footfall has an effect on the rent and vacancy rate. It follows that the effect of footfall on the logarithm of rental income can be written as the sum of the marginal effect of footfall on the logarithm of rent and the marginal effect of footfall on the level of the vacancy rate:

$$\frac{\partial logI}{\partial f} = \frac{\partial logp}{\partial f} - (1+\kappa)\frac{\partial v}{\partial f} \tag{5.2}$$

In our econometric framework, we will estimate the marginal effects of footfall on the logarithm of rent, as well as on the level of the vacancy rate. In (5.2), the value of κ will be assumed. This is not problematic, because we will see that if the effect of footfall on log rent is positive and the effect on the vacancy rate is negative, then we know that the effect of footfall on log rental income exceeds $\partial logp/\partial f - \partial c/\partial f$. But what are the signs of $\partial p/\partial f$ and $\partial v/\partial f$, according to theory?

5.2.2 A search model of a shopping street

Let us introduce a search model of a shopping street with two types of homogeneous agents. Property owners that possess properties and retail firms, which rent properties from property owners. When a property is occupied, a property will be labelled

[10]As an alternative, one may estimate the effect on transaction prices of stores. There are two reasons we prefer to focus on rental income. First, transaction prices reflect expectations about future rents. Second, sales transactions are rare relative to rent transactions. In our data, only 10 percent of the observations refer to sales transactions.

as a *shop*. Property owners with vacant properties and retail firms aiming to open a shop have to search for each other. Property owners set the level of advertising expenditure which determines the contact rate with retail firms. Given a contact, the agents use Nash bargaining to determine the rent level. We assume steady-state and a given number of store owners N, which possess one property each, which they aim to rent out to retail firms for rent p. For simplicity, the revenue of a shop is fully determined by footfall in the street. For now, we assume that the number of store owners and footfall are exogenous. The future is discounted at rate r. Owners and retail firms maximise their profits.

Retail firms go bankrupt at a given rate δ, which creates vacant properties. Owners with a vacancy and retail firms which aim to open a new shop search for each other. The rate at which they find each other is defined by a concave matching function m. This matching function depends positively on the overall advertising expenditures, ev, i.e. the number of vacant properties v times advertising expenditure e per property. Thus, $m = m(ev)$. Vacant properties become occupied at a rate $q(v,e)$, defined by $m(ev)/v$. This rate depends negatively on v, due to the concavity assumption of the matching function. Owners with a vacancy incur advertising costs $c(e)$. Advertising cost is an increasing convex function of advertising expenditure, whereas $c(0) = 0$. When an owner with a vacancy and a searching retail firm meet each other, they bargain about the shop price p, given a bargaining parameter β, where $0 < \beta < 1$. Rental income of the property owner is equal to $p(1-v)$.

The market for retail firms is competitive with free entry of searching retail firms, so the expected profit of searching retail firms is equal to zero. Property owners with vacancies choose their advertising expenditure conditional on the advertising expenditure of other property owners. We consider symmetric equilibria where owners choose the same advertising expenditure. The latter implies that for the representative owner, the marginal increase in the matching rate of advertising expenditure is equal to the average rate, so $\partial m/\partial e = m/e$. Similarly, $\partial m/\partial v = m/v$.

In steady-state, the inflow rate of shops is equal to the outflow rate, implying that:

$$m(ev) = \delta(1-v) \tag{5.3}$$

The present-discounted value of expected profits of a vacant property, V, can be written as:

$$rV = -c(e) + \frac{m(ev)}{v}(R-V) \tag{5.4}$$

where R denotes the present discounted value of expected profits of a property that is rented out. The latter can be written as:

$$rR = p + \delta(V - R) \tag{5.5}$$

The present-discounted value of expected profits for a retail firm with a shop equals:

$$rS = f - p - \delta(S - Q) \tag{5.6}$$

Retail firms that yet did not find a store to locate in have the following present-discounted profits Q:

$$rQ = -z(\eta) + \lambda(S - Q) \tag{5.7}$$

where $z(\eta)$ are search costs and η is search effort of retail firms and λ indicates the chance that a retail owner finds a store. Because of a competitive market, η is chosen optimally and Q will be equal to zero.

Nash bargaining implies that the property owners' share β of their own surplus, $R - V$, is equal to the retail firms' share, $(1-\beta)$, of their own surplus S. Consequently:

$$(1-\beta)S = \beta(R - V) \tag{5.8}$$

These four equations, combined with the first-order condition of (5.4) that the present-discounted value of expected profits of a vacant property is maximised with respect to advertising expenditure $c(e)$, imply that in equilibrium, p, v, e are determined by the following three equations:

$$p = \frac{f(1-\beta)(v(r+\delta) + m(ev)) - (r+\delta)v\beta c(e)}{(1-\beta)m(ev) + v(r+\delta)} \tag{5.9}$$

$$v = 1 - \frac{m(ev)}{\delta} \tag{5.10}$$

$$c'(e) = \frac{(1-\beta)(f + c(e))m(ev)}{erv + e\delta(1-(1-v)\beta)} \tag{5.11}$$

We are interested in the effects of footfall on prices and vacancy rates. Using (5.9), it is easy to see that the partial derivative $\partial p / \partial f > 0$. Although interesting, we are mainly interested in general equilibrium effects on prices and vacancy rates, taking into account the effects through changes in advertising expenditure. We formulate the following proposition:

Proposition 5.1. *In equilibrium, (i) shop price depends positively on footfall and (ii) the number of vacancies depends negatively on footfall.*

Proof. See Appendix 5.8.1.

The model implies that the marginal effect of footfall on prices is positive, but always smaller than or equal to one (when $\beta = 0$, so when retail firms have all bargaining power, then $\partial p/\partial f = 1$. The intuition for the result that $\partial v/\partial f < 0$ is that property owners' opportunity cost of not filling a vacant store increases with footfall.

5.2.3 Footfall and external effects

Until now, we assumed that footfall is exogenous. However, footfall likely depends on the vacancy rate in the shopping street (the intensive margin) and the number of shops in the shopping street (the extensive margin).

Let us first assume that footfall in the shopping street depends on the vacancy rate in the street. We assume that footfall is proportional to the occupancy rate of shops. Hence, $f = (1-v)\bar{f}$, where $\bar{f}$ is the footfall generated when all shops are non-vacant. This assumption implies that there is a negative external effect of vacant shops, because a vacant shop reduces footfall. To investigate the effects of $\bar{f}$ on prices and vacancies, we make the simplifying assumption that $c = e^2/2$ so that $c''(e) = 1$. We then formulate the following proposition:

Proposition 5.2. *When footfall is proportional to the occupancy rate of shops, (i) $\partial p/\partial \bar{f} > \partial p/\partial f$ and (ii) $\partial v/\partial \bar{f} < \partial v/\partial f$.*

Proof. See Appendix 5.8.2.

The main consequence of this proposition is that one underestimates the effect of log footfall on rental income when endogeneity is ignored. In other words, in the empirical application, the endogeneity concerns arising by the fact that footfall depends on vacancy rates are conservative. As we will show later, the underestimate is very small if vacancy rates are low, which is the case in our data.

It might also be that the number of stores in the shopping street is endogenous and depends on $\bar{f}$. Suppose that the number of shop buildings is endogenously determined in a competitive market, where the marginal benefit of owning a shop is equal to the rental income I. Let us further assume that the per period marginal construction and maintenance costs for a shop are equal to π. Furthermore, suppose that footfall is an increasing function of the number of shops N, so $\partial \bar{f}/\partial N > 0$[11]. It should hold that:

$$p(N)(1-v(N)) = \pi + cv(N) \tag{5.12}$$

[11] Arguably, shops are also able to increase footfall (e.g. by advertising), but we will ignore here this intensive margin.

Hence, because $\partial p/\partial \bar{f} > 0$ and $\partial v/\partial \bar{f} < 0$, the effects with an endogenous number of shops must be larger than if N is given.

5.2.4 Welfare and retail policies

Let us now focus on welfare and investigate whether certain policies would be welfare improving. An important policy question is whether an unregulated market leads to the optimal concentration of shops in a shopping street[12]. We will distinguish between subsidies given to store owners and subsidies to retail firms.

The welfare generated in a shopping street is equal to $N(I-\pi)$. Maximisation of welfare with respect to the number of shops implies that $I-\pi+N(\partial I/\partial N)=0$, whereas the marginal store owner will ignore the last term. Hence, the marginal external benefit of opening a shop is equal to:

$$N\frac{\partial I}{\partial N} = I \cdot \varepsilon_{I,N} > 0 \tag{5.13}$$

where $\varepsilon_{I,N}$ denotes the elasticity of rental income with respect to the number of shops. The Pigouvian subsidy to the marginal store owner must then be equal to $\varepsilon_{I,N}$ times the rental income of a shop. In our empirical analysis, we will estimate $\varepsilon_{I,N}$.

Let us now assume that shops are heterogeneous in the amount of footfall they generate. This immediately implies that it is not optimal to give the subsidy to a store owner (independent of her level of footfall), but that one may give different levels of subsidy to retail firms depending on the amount of footfall they generate. This is particularly relevant when new retail firms apply for (implicit) subsidies at the time they open a store, based on the argument that they will (substantially) increase footfall for other firms in the vicinity. Hence, let us assume that for certain retail firms it is known how much footfall they generate[13]. It is then useful to write the above equation as:

$$N\frac{\partial I}{\partial N} = N\frac{\partial I}{\partial \bar{f}}\frac{\partial \bar{f}}{\partial N} = I \cdot \varepsilon_{I,f} \cdot \varepsilon_{f,N} > 0 \tag{5.14}$$

Hence, $\varepsilon_{I,N}$ has been written as the product of the elasticity of the rent with respect to footfall, $\varepsilon_{I,f}$, and the elasticity of footfall with respect to the number of firms, $\varepsilon_{f,N}$, where the latter is retail-firm specific. In our empirical application, we will

[12] Another question is whether property owners with vacancies choose the optimal advertising expenditure. Hosios (1990) shows that in the labour market, the policy consequences of answering this question are not very clear. We leave this question for further research.

[13] Retail firms may also differ in the extent that they benefit from footfall $\partial I/\partial f$. However, the latter margin is not external to the decision to build a shop, so here we assume that firms are only heterogeneous with respect to the amount of footfall they generate for other firms.

estimate $\varepsilon_{I,f}$. Given the specific values for $\varepsilon_{f,N}$, we calculate the optimal subsidy to attract the marginal retail firm. Note that $\varepsilon_{I,f}$ is also of interest, when policy makers are able to influence footfall directly (e.g. through subsidised car parking).

We emphasise here that our data indicate that shopping externalities are not internalised in shopping streets. On the other hand, in shopping malls, developers will internalise these externalities by determining the optimal number of stores and by charging lower rents to footfall-generating shops (or 'anchor stores') (Brueckner, 1993; Pashigian and Gould, 1998; Konishi and Sandfort, 2003). Therefore, in a shopping mall, the developer is able to provide first-best subsidies, based on the amount of footfall generated by each store, and maximize mall's welfare.

5.3 Econometric framework, data and descriptive statistics

5.3.1 Econometric framework

We first focus on the estimation of the effect of shopping externalities on rents of retail establishments. Let p_{ijt} be the rent paid by retail firm i at location j in year t. Furthermore, let f_{jt} be the footfall at each shop location j within a shopping street (defined in Section 5.3.1) and z_{ijt} other property and location characteristics (e.g. shop size, construction year, historic district). The basic equation to be estimated yields:

$$logp_{ijt} = \alpha log f_{jt} + \gamma z_{ijt} + \vartheta_t + \epsilon_{ijt} \tag{5.15}$$

where α and γ are parameters to be estimated, ϑ_t are year fixed effects and ϵ_{ijt} is an identically and independently distributed error term.

There are four major concerns when interpreting α as a causal estimate of shopping externalities. The *first* concern is that the estimated effect of footfall is causal, but that a location may also attract pedestrians that use the shopping street with no interest in shopping (non-shoppers). In particular, footfall levels are usually higher close to railway stations, because workers who commute by train may walk from the railway station to their work/home. Hence, if footfall is measured with error, it may not necessarily capture shopping externalities. This concern turns out to be minor because we use observations of footfall, which were collected on Saturdays for the main shopping streets of the Netherlands. For this sample of observations, almost all pedestrian movements are attributed to shopping. It is therefore very unlikely that any measurement error is substantial or systematic[14]. Even in the case of a non-systematic measurement error, the bias in our estimates is expected to be limited, as non-shoppers aim to avoid crowded shopping streets[15].

The *second* concern is that the estimated effect of footfall is causal, but a location may also attract shoppers that use the shopping street to visit *one specific shop* with no interest in visiting other shops on the same street, so-called 'one-stop shoppers'. One-stop shoppers do not generate any shopping externality, although they may be included in our measure of footfall. Our identification strategy, which focuses in differences of footfall within very small areas addresses this issue. Any spatial difference in the share of one-stop shoppers would most likely lead to a bias in the estimated effect of the shopping externalities because of measurement error bias. We use an example to show that this measurement error is expected to be small even if the share of one-stop shoppers is substantial. Note that the probability that a one-stop shopper is included in our measure of footfall is (approximately) proportional to the number of shops visited. For example, if 25 percent of footfall were one-stop shoppers, and the other 75 percent visit four shops, then the proportion of one-stop shoppers would be only 7.8 percent[16]. Consequently, any measurement error because of one-stop shoppers is expected to be limited.

The *third* concern may rise because footfall data are collected only on two Saturdays per year as we will explain in detail in Section 5.3.1. Thus, the annual measure of footfall may suffer from measurement error due to the random variation between different Saturdays of each year. Annual variation in our measure of footfall at the same location is thus likely to be substantial, even when actual annual variation in footfall is absent. Identification based on annual differences would lead to a downward bias in the estimated effect of footfall if this is the case. In contrast, spatial variation in our measure of footfall due to random sampling error is likely minimal, because different locations in close proximity are measured on the same day. Hence, identifying the effect of footfall using spatial variation in local footfall addresses such measurement error concerns[17].

The *fourth* and main concern refers to the presence of unobserved location char-

[14]On average, about 60 percent of all pedestrian movements in cities are attributed to shopping (and, for example, only 7.5 percent to commuting) (Statistics Netherlands). By focusing on Saturdays, our measure of pedestrians hardly includes any commuters.

[15]In a robustness check, we will show that by excluding observations close to train stations, our results remain robust.

[16]Furthermore, it is plausible that one-stop shoppers aim to avoid walking through busy shopping streets, and do not enter the shopping street at a random location, but from a side road which is close to the shop they want to visit. This makes it even more likely that one-stop shoppers are less than proportionally included in our measure of footfall.

[17]In a sensitivity analysis, we use the annual average of footfall, as well as the footfall of the previous year, as the main variable of interest. We obtain similar results.

acteristics that are correlated with footfall. For example, building quality may be important for profits. When building quality is non-randomly distributed over space (e.g. nicer buildings in areas with more footfall) and customers value building quality, a naïve hedonic regression will suffer from bias. Also, zoning and other regulations may force retail firms to locate at more expensive locations with more footfall (Cheshire et al., 2015). When one does not account for characteristics that cause omitted variable bias, one is likely to overestimate the importance of shopping externalities[18].

To control for unobserved locational endowments, we take a number of steps. First, we include shopping district or shopping street fixed effects, implying that we identify the differences in footfall within the shopping district or the shopping street, respectively. This approach mitigates the problem of unobserved endowments, but may not solve the problem entirely because shopping streets may be quite long (up to 1,269m). We therefore also propose another identification strategy using spatial variation in local footfall between intersecting streets (because people follow certain routes for their shopping trips).

Our idea is to compare shops that are very close to street intersections (e.g. within 100 or 50m). Locations close to intersections are arguably identical in unobserved spatial components, such as local policies, nearby parking etc. Let d_{jn} be the distance of shop at location j to the nearest intersection n in metres and ϕ_n captures a set of intersection fixed effects, i.e. dummies that equal one when j is within $\bar{d}$ distance of intersection n. We then estimate:

$$logp_{ijt} = \alpha log f_{jt} + \gamma z_{ijt} + \phi_n + \vartheta_t + \varepsilon_{ijt}, \text{ if } d_{jn} < \bar{d} \tag{5.16}$$

One may argue that the estimate of footfall based on (5.16) may still suffer from omitted-variable bias, because intersecting streets may have different unobserved characteristics which are relevant for both footfall and rent. Hence, we have constructed a range of street and shop characteristics (for details see Section 5.3) that we denote as x_{ij}. In particular, street width seems relevant, because smaller streets may restrict footfall and imply less visibility. We therefore calculate for each shop the distance to the opposite side of the street and we also include a dummy indicating whether a street is pedestrian.

Another potential issue could be that corner shops have two shop windows in two different streets, therefore benefiting from pedestrians passing in either street,

[18]Different solutions have been proposed to address these endogeneity issues of agglomeration. Many studies use long-lagged instruments (Ciccone and Hall, 1996; Melo et al., 2009). However, there is extreme persistence of shopping streets over time. This makes it plausible that unobserved endowments that were important a century ago are still affecting current rents of shops. Hence, long-lagged instruments may be invalid in this setting.

whereas our measures always refer to one street only[19]. Furthermore, shops located inside a shopping mall are expected to have different footfall and pay different rent than the shops located on the street[20]. Finally, one might also argue that a shop would pay a higher rent to be on the sunny side of the street while sun also attracts more pedestrians. We thus created dummy variables for corner shops, for shops inside a mall and for shops located on the sunny side of the street. We then include these additional shopping street and shop characteristics x_{ij} in the regression:

$$logp_{ijt} = \alpha log f_{jt} + \beta x_{ij} + \gamma z_{ijt} + \phi_n + \vartheta_t + \epsilon_{ijt}, \text{ if } d_{jn} < \bar{d} \tag{5.17}$$

where β are additional parameters to be estimated.

In the current paper, we will not only estimate the effect of log footfall on log rent, but also on whether the shop is vacant, indicated by v_{ijt}. We will then use the same approach as described above to address endogeneity issues. However, one may argue that there is reverse causality because footfall may be dependent on the vacancy rate in a neighbourhood. To address this issue, we use an insight provided by our theoretical framework and write $f_{jt} = (1 - v_{jt})\bar{f}_{jt}$, where v_{jt} is the vacancy rate in location j in year t and $\bar{f}_{jt}$ is the footfall generated by the non-vacant shops in location j in year t. Therefore, in the parsimonious specification:

$$\begin{aligned} v_{ijt} &= \alpha log((1 - v_{jt})\bar{f}_{jt}) + \beta x_{ij} + \gamma z_{ijt} + \phi_n + \vartheta_t + \epsilon_{ijt} \\ &= \alpha log(1 - v_{jt}) + \alpha log \bar{f}_{jt} + \beta x_{ij} + \gamma z_{ijt} + \phi_n + \vartheta_t + \epsilon_{ijt}, \text{ if } d_{in} < \bar{d} \end{aligned} \tag{5.18}$$

If shops within location j are identical and because v_{jt} is small, it holds that $log(1 - v_{jt}) \approx -v_{jt}$. This implies that:

$$v_{ijt} = \frac{\alpha}{1+\alpha} log \bar{f}_{jt} + \beta x_{ij} + \gamma z_{ijt} + \phi_n + \vartheta_t + \epsilon_{ijt}, \text{ if } d_{in} < \bar{d} \tag{5.19}$$

When α is small (which appears to be the case), one immediately observes that $\alpha \approx \alpha/(1+\alpha)$, so the problem of reserve causality does not seem to be important here[21].

[19] We also expect some measurement error in footfall at shopping street intersections.

[20] About 4 percent of our shop observations are 'inside malls', defined in the next section. The results are identical when we exclude these observations (see Appendix 5.8.4).

[21] We also address this issue by using footfall in the previous year(s) as the main variable of interest in the sensitivity analysis in Appendix 5.8.4. The results do not change.

5.3.2 Data

We base our empirical analysis on six datasets. The first one is obtained from Strabo, a consultancy firm that gathers commercial property data. It comprises transactions of commercial properties provided by real estate agents from 1986 to 2015. The dataset contains information about annual rents and rental property attributes, such as address, size (gross floor area in m^2) and whether the building is newly constructed or renovated. From the Strabo dataset, we exclude observations for which no rent is reported. These observations comprise 27.8 percent of all shops in the Strabo dataset. The rental transactions are then matched to data from the Administration of Buildings and Addresses, which provides the exact location and construction year for all buildings in the Netherlands. Using a 5m distance threshold, we matched 72.9 percent of the Strabo shops. The distance between a shop location and the nearest building is zero for 90 percent of the matched shops. Based on the Listed Building Register, we have added information on whether the rental property is in an area that is assigned as a historic district. The latter is relevant since historic districts may attract tourists that are (not) interested in shopping. The dataset is also merged with detailed land use data from Statistics Netherlands. The latter data enable the calculation of distance to the nearest water body and to the nearest train station[22].

The fifth dataset is a retail dataset obtained from Locatus, which contains the *entire population* of retail establishments. For each retail establishment, we know whether the shop is vacant or occupied and the retail sector (when occupied), and whether a shop is part of a chain.

The Locatus dataset also provides 3,936 counts of footfall in all *main shopping streets* of the Netherlands from 2003 to 2015 (these shopping streets contain about 13.4 percent of all shops in the Netherlands). The annual footfall data, provided by Locatus, is the average footfall collected on two 'regular' Saturdays in Spring and two Saturdays in Autumn at four different hours of the day at many different locations close to shops in all main shopping streets of the Netherlands[23]. Using these measurements, Locatus calculates the average footfall per day, which represents the average number of shoppers per day. The footfall data are matched to all shops in the previously-defined shopping streets. Within each shopping street, the average distance between footfall measures is approximately 45m.

We have defined a *shopping street* as a continuous straight street (or slightly

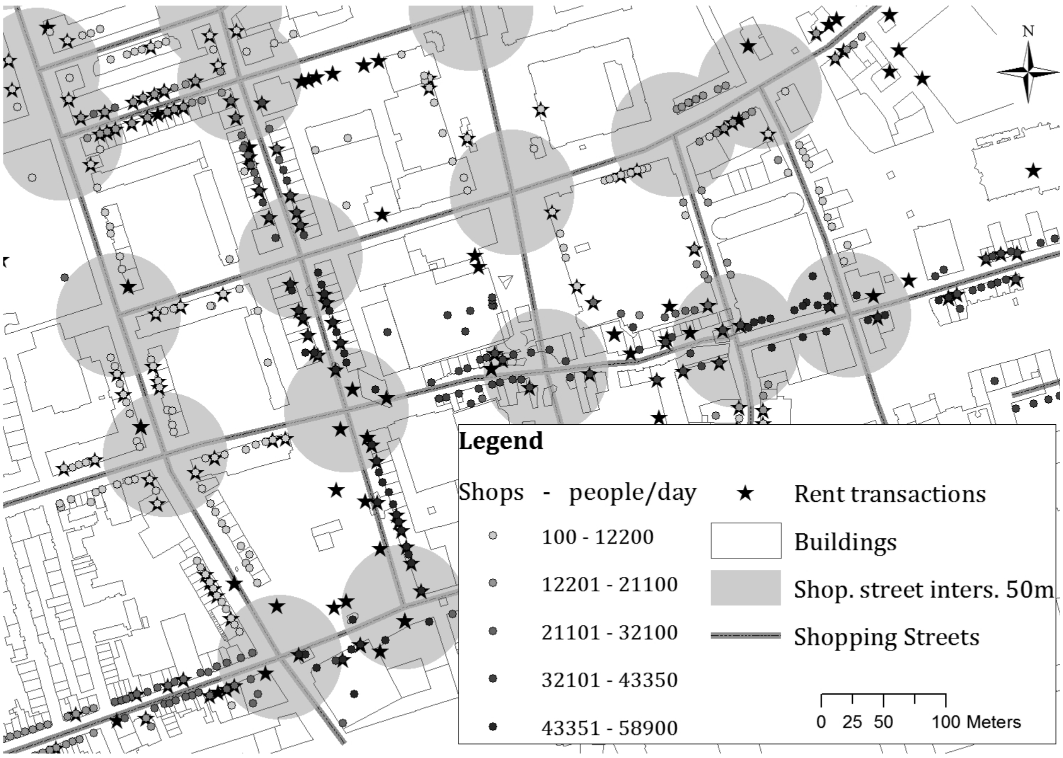
[22]Water bodies in the Netherlands are mainly canals, which are often attributed with some aesthetic value. Therefore, it is important to control for the attractiveness of such locations.

[23]'Regular Saturdays' do not coincide with holiday periods (e.g. Easter) and are not preceded by bank holidays. Furthermore, on these days there is no heavy rainfall or other extreme weather conditions. The average distance of each shop to a measurement point is approximately 29m.

curved) based on manually created GIS polyline shapefiles for all streets for which there is at least one location where footfall data are available. Using this definition, based on the above-discussed Administration of Buildings and Addresses dataset, we define 1,160 unique shopping streets.

Given the points of intersection between shopping streets, we calculated the distance from each shop to its closest shopping street intersection. We have also used information from OpenStreetMap in order to determine if a shopping street is pedestrian. We also determine the street width, which is calculated using the average distance to the four closest buildings from the building in which each shop is located. We have set the minimum width at 3m, which applies to a few small alleys in historic districts. We also created a dummy variable for the shops located inside a mall, defined here as the shops which are in the interior of buildings. In addition, we have constructed a corner shop dummy variable for shops located within 10m from an intersection and a sunny side of the street dummy variable if the orientation of a shop is towards the south[24]. Finally, we used a distance threshold of 25m to match each Strabo shop to the nearest shopping street, as defined above.

Figure 5.1: Sample map for the Rotterdam city centre



We have also matched each rent transaction in the Strabo dataset to a shop in the

[24]For corner shops, we also used alternative distance thresholds of 25m and 50m and the results are virtually unchanged.

Locatus dataset. In order to recover more information on shop individual characteristics, we have matched each shop from the Strabo dataset to its corresponding shop in the Locatus dataset. Specifically, the matching is based on different combinations of building identifiers, the full shop names or the first letters of the names, the address numbers or the postal codes. For details see Appendix 5.8.3. It should be mentioned that our main results are not sensitive to this matching process.

We illustrate the data and identification strategy in Figure 5.1 based on a sample of our data for the city centre of Rotterdam. As it can be seen by the level of footfall in different shop locations, there is substantial spatial variation in the annual average of footfall both *within* shopping streets and *between* intersecting shopping streets. Moreover, rent transactions (the stars in the map) are numerous and cover almost the whole area that we have information on footfall. Therefore, we can use both the within and between shopping street variation in footfall and retail rents to identify the external effect of shopping.

5.3.3 Descriptive statistics

In this Section, we present the descriptive statistics for the main variables that we include in our analysis. Our main dependent variable is the annual rental price. Table 5.1 summarises the descriptive statistics for the Strabo dataset. We have 3,102 rental transactions located on 682 different shopping streets with 831 shopping street intersections. We show that the rental price has a mean of €51,449. Our main independent variable of interest, footfall also exhibits substantial variation, which ranges from 200 to 79,000 pedestrians passing by a certain point each day. The mean daily footfall is 13,552 people with a standard deviation of 10,724. The majority of shops are relatively small, with a mean of 190m^2 and a median of 135m^2. Few shops are located inside a mall (3.7 percent) or on the corner of two shopping streets (3 percent) while about half the shops of our sample (48 percent) are located on the sunny side of the street. We also have information on the total building surface area and other building characteristics. About one percent of buildings are either new or renovated when the rental transaction took place. It is not too surprising for European shopping streets that roughly 78 percent of the shops in our sample are located in pedestrian streets, about half the shops are in buildings constructed before the Second World War and a similar share is located within historic districts.

The average distance to the nearest train station is 1.2km, but the median distance is much shorter and only 747m (hence, there is good railway accessibility)[25].

[25] For some shops, the distance to the nearest station may have decreased over time because between 2003 and 2013, 29 new, but small, stations (from 370 to 399) were opened, mainly in residential areas.

Shopping street length ranges from approximately 44 to 1,270m with a mean and median of 431 and 359m, respectively. Street width is on average 8.1m. In each shopping street, there are about 70 active shops on average. In our data, we will also distinguish between 153 shopping districts (a shopping district contains about 20 rent transactions and 260 shops, on average).

Table 5.1: Descriptive statistics of Strabo dataset

	mean	sd	min	max
Rent (€/year)	51,449	73,163	4,800	2,700,000
Rent per m^2	322.8	220.5	30	3,000
Footfall (potential shoppers per day)	13,552	10,274	200	79,000
Size of property (in m^2)	190.4	206.1	25	4,000
Building surface area (in m^2)	1,275	4,490	20.44	86,771
Building - new	0.00387			
Building - renovated	0.00645			
Sublet property	0.00613			
Construction year < 1940	0.565			
Construction year 1940-1949	0.0193			
Construction year 1950-1959	0.0883			
Construction year 1960-1969	0.0516			
Construction year 1970-1979	0.0609			
Construction year 1980-1989	0.0645			
Construction year 1990-1999	0.0758			
Construction year $\geq$ 2000	0.0587			
Construction year missing	0.0161			
Mall	0.0374			
Corner shop	0.0297			
Sunny side of street	0.481			
Pedestrian street	0.7795			
Shopping street length (in m)	430.8	270.5	43.92	1,269
Shopping street width (in m)	8.116	5.605	3	38.44
Number of (non-empty) shops in shopping street	70.705	51.521	2	227
Distance to nearest intersection (in m)	67.03	87.21	0.684	988.3
Water within 50m	0.0461			
Water 50-100m	0.0687			
In historic district	0.478			
Distance to station (in m)	1,206	1,928	65.97	18,280

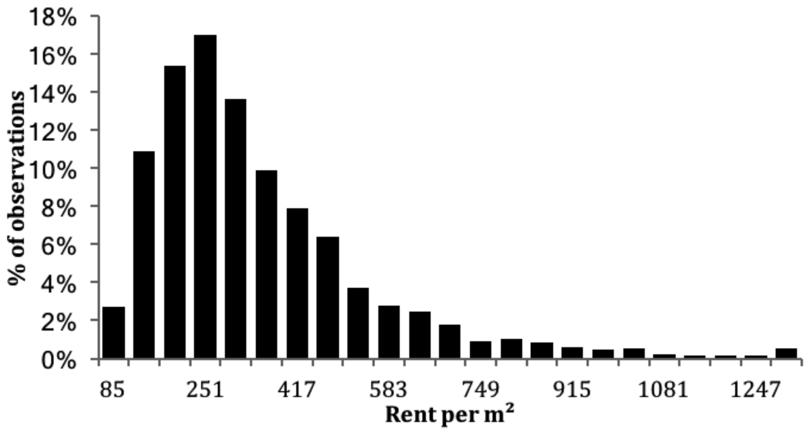
Notes: The number of observations is 3,102.

A substantial proportion of shopping districts (about 45 percent) are not within 5 km of the centre of a city[26]. Hence, in terms of shopping districts, we have a good representation of non-city centre shopping districts. However, the proportion of *shops* not within 5 km of the centre of a city is much smaller and only 23 percent because suburban shopping centres tend to be smaller.

[26]We define centres of all cities in the Netherlands with at least 50,000 inhabitants.

Figure 5.2 shows an histogram of rent per m^2. About 57 percent of the observations are in the range €82-307, while the distribution of observations suggests that a logarithmic specification should fit well the data.

Figure 5.2: Rent histogram



In Table 5.2, we report descriptive statistics for shops in the Locatus dataset. We have 416,675 shop observations in 161 shopping districts, 1,160 shopping streets and near 1,395 shopping intersections. About 6 percent of the shops are vacant. It appears that the Strabo dataset contains a considerably higher share of shops in older buildings (particularly constructed before 1940) than the full population. The main explanation is likely to be that the Strabo dataset is based on rental *transactions*. Therefore, it is not a random sample of the population of shops because owned shops are not included and shops with long rental contracts are underrepresented. This suggests that our results for footfall may not extend to newly built owned shops. The descriptive statistics of the location variables are however comparable to the descriptive statistics for the Locatus data.

Our sample of shops is clearly not a random sample of shops nationwide, as we focus on shops in shopping streets that aim to profit from footfall. In particular, most shops in our sample are clothing shops (29 percent), which are strongly over-represented compared to the national average (about 8 percent in the Netherlands). However, the share of restaurants and cafes, which is the second more common sector in our sample is fully representative for the full population of shops (16 percent in both our sample and in the whole population). Each of these sectors typically comprises shops that sell close substitutes (while the branches as a whole are complementary) although the degree of product differentiation in these sectors is arguably high.

In Europe, shopping districts usually exhibit a pattern of mixed land uses. In line with this, using information from the Administration of Buildings and Addresses dataset for buildings within 25m of a shopping street, it appears that almost 50 percent of the properties is used by residents, about 25 percent for shopping and 25 percent for other purposes (e.g. offices, public services).

Table 5.2: Descriptive statistics of Locatus dataset

	mean	sd	min	max
Property is vacant	0.0625			
Footfall	12,334	10,645	100	102,600
Size of property (in m^2)	175.3	1,078	1.637	27,694
Construction year > 1940	0.179			
Construction year 1940-1949	0.0136			
Construction year 1950-1959	0.0859			
Construction year 1960-1969	0.155			
Construction year 1970-1979	0.183			
Construction year 1980-1989	0.112			
Construction year 1990-1999	0.125			
Construction year $\geq$ 2000	0.147			
Mall	0.0612			
Corner shop	0.0249			
Sunny side of street	0.498			
Pedestrian street	0.7296			
Shopping street length	403.7	244.9	21.09	1,269
Shopping street width (in m)	12.99	10.94	3	50
Number of (non-empty) shops in shopping street	110.5064	88	0	572
Distance to intersection (in m)	87.59	170.7	2.097	3,808
Water within 50m	0.0509			
Water in 50-100m	0.0743			
In historical district	0.393			
Distance to station (in m)	1,583	2,754	1.98	18,534

Notes: The number of observations is 416,242.

We mentioned in the introduction that property ownership (and therefore land ownership) of shops is fragmented in city centres. This observation is based on the Strabo dataset for which the property owner type is reported. We know the property owner name for about one third of observations. It appears that on average only 18 percent of shops belong to property owners who own multiple properties in the same shopping street[27]. This evidence indicates that it is highly unlikely that the shopping externality that we measure is internalised. There is also information about property owner type that is available for about two thirds of the same sample. Property owner types are private-property owners, real estate agencies, pension funds, construction companies etc. We will use all this information in the sensitivity analysis.

[27]Given that this dataset only contains rental transactions, it is likely that the percentage of multi-property owners is overrepresented in our sample, because this percentage is likely lower for owned shops. Moreover, only 32 percent of shops are owned by companies, so the large majority of shops are owned by individual private investors.

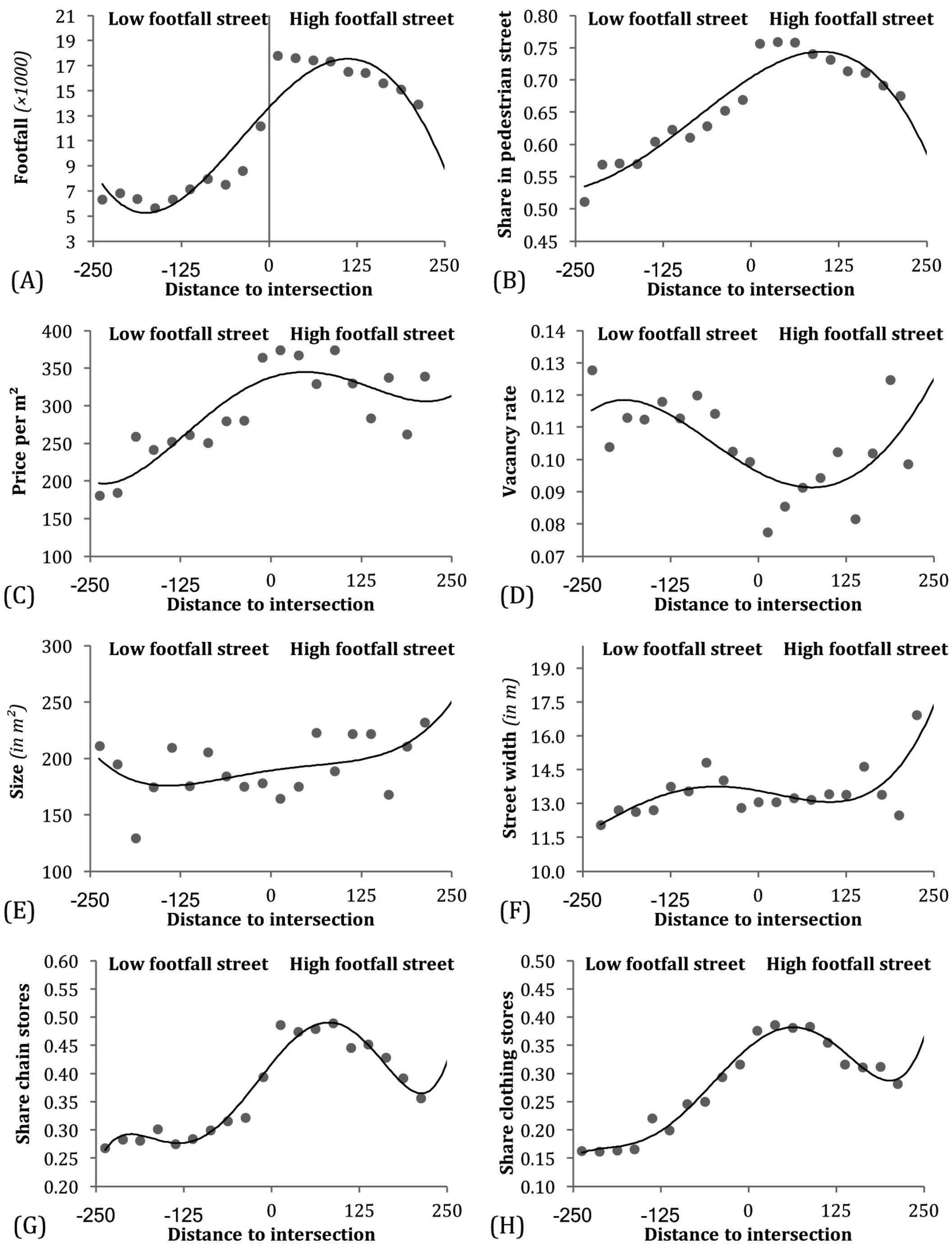
Our main identification strategy is based on spatial differences around shopping street intersections. Our basic assumption is that in close proximity to an intersection, shops located on intersecting shopping streets have common unobservable characteristics (e.g. local amenities, accessibility to public transport, parking spaces etc.). Therefore, conditional on property, location and other shop and street characteristics, we may identify the causal effect of shopping externalities.

We present here some graphical evidence. In essence, we show that in intersecting streets, footfall and rents depend on distance to the intersection in *a systematic and very similar way*, whereas e.g. the size of the property - which is one of the main observed determinants of retail rent - does not systematically depend on this distance. We constructed 25m bins for the distance between each shop and the nearest intersection. Negative distances denote shops located at low-footfall streets and zero distance denotes the intersection. We emphasise here that this is *not* a Regression-Discontinuity Design, because we do *not* need a discrete jump in footfall around street intersections but merely exploit the local variation in footfall close to these intersections. Hence, there should be considerable variation at the local level in the variables of interest. To construct these graphs, we exclude intersections where the difference in average footfall between two intersecting streets was minimal, i.e. below the first quartile of these differences (see similarly, Bayer et al. (2007)). We then regressed footfall, shop size, retail rents per m^2 and other measures, on 25m bin dummies for observations within 250m of the intersection and a spatial trend, while including intersection fixed effects to control for unobserved characteristics that are common to the intersecting streets. These dummy variables can be interpreted as conditional means. These graphs also allow us to investigate whether shopping streets with high-footfall are distinctively different from low-footfall streets.

Figure 5.3 reports the results. In Panel A, it is shown that, by construction, footfall is considerably lower at the low-footfall street close to the intersection distance. Footfall is already higher close to intersections in the low-footfall street, which may be due to corner shops that have access to both streets. In Panel B, it can be seen that the share of pedestrian streets is highly correlated with footfall, which is not too surprising. Later, we will show that if we control for pedestrian streets, the impact of footfall is hardly affected.

In Panel C, Figure 5.3, we observe that there is also considerable variation in prices close to the intersection. For example, the annual price per m^2 is about €280 in the low-footfall street, while in the high-footfall street is it about €365, which is a considerable increase. The variation in vacancy rates is less clear-cut (Panel D), but we can still observe a lower average vacancy rate in the high-footfall street.

Figure 5.3: Variation near intersections



Notes: In Panels C and E we use Strabo data. In the rest of the panels, we use Locatus data. The spatial trend is estimated by a third-order polynomial of the variable of interest on the distance to the closest intersection.

One may argue that high-footfall streets are distinctively different from low-footfall streets. We do not find strong evidence for this claim; both shop size and the street width do not show substantial variation around the intersection point (see Panels E and F). On the other hand, we find evidence for sorting; it seems that chain stores, which are often clothing stores, are located at the high-footfall streets (see Panels G and H). As we mentioned in the introduction, clothing shops are expected to benefit strongly from footfall as people searching for clothes often browse through shops and engage in trip-chaining. Therefore, clothing shops are expected to sort into high-footfall streets and pay a higher rent. Nonetheless, in a sensitivity analysis we show that chain and non-chain stores have an identical preference for footfall. Hence, this sorting is unlikely to drive our results.

5.4 Results

5.4.1 Effects of footfall on rents

Table 5.3 reports the results of our baseline regressions. The specification in Column [1] is an ordinary least squares (OLS) regression of the log rental price on log footfall, log size of the rental property, building and location characteristics, in line with equation (5.15) in Section 5.2. The elasticity of footfall with respect to rental price is 0.32. The coefficients related to property and building attributes have the expected signs and magnitudes[28].

The specification in Column [1] might suffer from omitted variable bias due to the omission of unobserved features of a shop location that are correlated with footfall. For example, some shopping areas are more attractive due to their proximity to a museum, school or other neighbourhood-specific amenities. The relevance of such factors is clear from the positive (and statistically significant) coefficient of the historic district dummy. A partial solution to this problem is the inclusion of shopping district fixed effects in Column [2]. This may mitigate some of the aforementioned endogeneity issues. Although the coefficient of footfall remains virtually unchanged, unobservable characteristics at a smaller spatial scale might still cause omitted variable bias. For this reason, we also include shopping street fixed effects in Column [3], Table 5.3. In this specification, we essentially exploit the variation in footfall *within* shopping streets. Although the estimated standard error of the footfall coefficient in Column [3] is substantially larger, the estimated coefficient is only slightly lower.

[28]There is one exception; distance to the nearest station has a counterintuitive sign, because it is positively correlated with attractive unobserved features of location such as city size. Indeed, it becomes negative (but statistically significant) in the more believable specifications.

Table 5.3: Regression results for retail rents

Dependent variable:	log(rent)					
	[1] OLS	[2] OLS	[3] OLS	[4] OLS	[5] OLS	[6] OLS
Footfall (log)	0.322[a]	0.307[a]	0.306[a]	0.230[a]	0.215[a]	0.213[a]
	(0.0217)	(0.0181)	(0.0300)	(0.0270)	(0.0349)	(0.0350)
Size of property in m^2 (log)	0.588[a]	0.607[a]	0.608[a]	0.628[a]	0.622[a]	0.622[a]
	(0.0186)	(0.0138)	(0.0177)	(0.0172)	(0.0243)	(0.0244)
Building surface area in m^2 (log)	0.0422[a]	0.0332[a]	0.0351[a]	0.0517[a]	0.0571[a]	0.0596[a]
	(0.00930)	(0.00879)	(0.0126)	(0.0127)	(0.0162)	(0.0196)
Building - new	0.00320	-0.0613	0.0617	0.0614	-0.0793	-0.0758
	(0.161)	(0.142)	(0.111)	(0.137)	(0.282)	(0.287)
Building - renovated	0.417[a]	0.334[a]	0.246[a]	0.209[c]	0.115	0.114
	(0.1000)	(0.0764)	(0.0750)	(0.107)	(0.0911)	(0.0910)
Sublet property	-0.0205	-0.00777	-0.0633	-0.105	-0.156	-0.150
	(0.0870)	(0.0760)	(0.0980)	(0.0991)	(0.0982)	(0.0966)
Property is in mall						-0.0392
						(0.128)
Property on the corner						0.0672
						(0.0536)
Property is on sunny side of street						-0.0185
						(0.0261)
Shopping street width in m (log)						-0.00793
						(0.0454)
Pedestrian street	0.0469	0.118[a]		0.0125	0.0519	0.0552
	(0.0358)	(0.0319)		(0.0501)	(0.0700)	(0.0701)
Water within 50m	-0.0323	-0.158[a]	-0.127[c]	-0.0122	0.0280	0.0265
	(0.0507)	(0.0525)	(0.0684)	(0.116)	(0.109)	(0.112)
Water 50-100m	0.0356	-0.0613[c]	-0.105[b]	0.0394	0.0368	0.0438
	(0.0553)	(0.0368)	(0.0424)	(0.0606)	(0.0831)	(0.0861)
In historic district	0.0676[c]	-0.0513	0.0613	0.0623	0.0305	0.0387
	(0.0366)	(0.0752)	(0.102)	(0.0695)	(0.0952)	(0.0967)
Distance to station (log)	0.0602[a]	0.0382	0.0193	-0.0373	-0.0363	-0.0378
	(0.0183)	(0.0445)	(0.0681)	(0.0598)	(0.0495)	(0.0477)
Construction year dummies	✓	✓	✓	✓	✓	✓
Year fixed effects	✓	✓	✓	✓	✓	✓
Shopping district fixed effects		✓				
Shopping street fixed effects			✓			
Intersection fixed effects				✓	✓	✓
Observations	3,102	3,102	3,102	2,629	1,870	1,870
R^2	0.582	0.711	0.809	0.848	0.871	0.872

Notes: Footfall is measured as the number of shoppers per day. In Column [4], we include observations within 100m of a shopping street interaction. In Columns [5] and [6], we reduce this distance to 50m. Robust standard errors clustered at the shopping street level are in parentheses. ^{a}, ^{b} and ^{c} indicates significant at 1, 5, and 10 percent level, respectively.

In our sample, the shopping street length is about 400m, on average, but can be more than 1 km, suggesting that there may still be unobservable factors that vary within shopping streets which induce endogeneity issues. Examples include a small square with a fountain, a sculpture located in the middle of a street or a nice view. In order to deal with such factors, Columns [4]-[6] exploit variation between shops that are within a given distance of the same intersection. In Column [4], we include fixed effects for the shop locations that are within a distance of 100m from an intersection, while in Column [5], we reduce the distance to the nearest intersection to 50m. It is plausible that shops located very close to an intersection are essentially identical, when we control for property and building characteristics. Columns [4] and [5] show that when we use this identification strategy, the estimated coefficient for footfall is reduced to 0.23 and 0.22, respectively.

Finally, one could argue that even when we compare the rental prices of shops on the different intersecting streets, street width may be an important omitted variable which could affect both footfall and rental prices[29]. The relationship between shopping width and footfall may be mechanical, because street width puts an upper bound on footfall. Moreover, shopping street width might affect the visibility of a shop, the supply of stock material, or the noise caused by pedestrians and cars in some cases. In Column [6], Table 5.3, we include in addition to 50m intersection fixed effects, shopping width, a dummy if a shop is located on a corner, another dummy if the shop is on the sunny side of the street, the logarithm of shopping street width, and another dummy if a location is inside a mall. The estimated coefficient for footfall is highly statistically significant and its elasticity is 0.21, virtually the same as in Column [5]. This implies that if we increase log footfall by one standard deviation, the increase in rent is then roughly 12 percent (0.56×0.21).

Let us now calculate the marginal effect of footfall. Recall that the average footfall on a typical Saturday is around 14,000, whereas average annual rent per m^2 for a shop is about €300. In general, footfall on Saturday is roughly one fifth of weekly footfall (Locatus, 2006). Let us increase footfall by one pedestrian in each day of the year. The annual increase in rent per m^2 is then approximately €0.00002[30]. Consequently, the monetary benefit of one additional pedestrian passing by a shop with an average size of almost $200m^2$ is estimated to be about €0.004[31].

So far, the analysis has focused on the effects of footfall on rents, as to estimate

[29] Street width is shop-specific and therefore not captured by shopping street fixed effects.

[30] This number is the product of the log footfall coefficient of Column [6], Table 5.3, (0.21) and the average annual rent per m^2 (€323 per m^2) divided by the product of the mean footfall (13,552), multiplied by 5 (because footfall on Saturdays is approximately one fifth of the weekly footfall) and by the number of weeks in a year (52).

[31] The order of magnitude of this result seems to make sense. Let us suppose that one out of hundred persons who pass a certain shop also enter that shop. Furthermore, assume that 25 percent of those who enter the shop also make a purchase and the profit per purchase is equal to €1.60. The marginal profit of footfall for a shop is then equal to $0.01 \times 0.25 \times 1.60$ = €0.004.

the elasticity of rental income with respect to footfall, $\varepsilon_{I,f}$. However, one may argue that the direct estimation of rental income with respect to the number of shops $\varepsilon_{I,N}$ is more interesting, as the marginal external benefits are equal to $I \cdot \varepsilon_{I,N}$ (see equation (5.13)). However, because the spatial extent to which shops in the vicinity contribute to footfall is unknown (and maybe different for different shop types), we think $\varepsilon_{I,f}$ is much easier to estimate than $\varepsilon_{I,N}$.

Table 5.4: Regression results for retail rents: Number of shops

Dependent variable:	log(rent)				
	OLS	OLS	OLS	OLS	OLS
	[1]	[2]	[3]	[4]	[5]
Number of shops in street (log)	0.0646^a	0.0789^a	0.115^a	0.0981^a	0.100^a
	(0.0241)	(0.0171)	(0.0223)	(0.0236)	(0.0234)
Property characteristics	✓	✓	✓	✓	✓
Building characteristics	✓	✓	✓	✓	✓
Location characteristics	✓	✓	✓	✓	✓
Shopping street characteristics					✓
Year fixed effects	✓	✓	✓	✓	✓
Shopping district fixed effects		✓			
Intersection fixed effects			✓	✓	✓
Observations	3,102	3,102	2,629	1,870	1,870
R^2	0.466	0.637	0.836	0.863	0.864

Notes: The number of shops in street (log) is the logarithm of the number of non-vacant shops on the same street and in the same year that the rent transaction took place. Property, building, location and shopping street characteristics are mentioned in Table 5.3. In Column [3], we include observations within 100m of a shopping street interaction. In Columns [4] and [5], we reduce this distance to 50m. Robust standard errors clustered at the shopping street level are in parentheses. ^{a}, ^{b} and ^{c} indicates significant at 1, 5, and 10 percent level, respectively.

Table 5.4 reports a similar set of specifications as in the previous table, while including the log number of shops on the shopping street and in the same year that the rent transaction took place, instead of log footfall[32]. Column [1] is the most parsimonious specification, Column [2] includes shopping district fixed effects and Column [3] includes 100m intersection fixed effects. We do not use street fixed effects because the number of shops in street variable does not exhibit any spatial variation within the shopping street by construction. In Columns [4] and [5], we restrict the sample to 50m from an intersection. The coefficient of log number of shops is statistically significant in all specifications. The coefficient of the log number of shops in Column [5], which also includes shopping street characteristics, is 0.1. Thus, a 10 percent increase in the number of shops in a street causes a 1

[32] We matched each rent transaction to each non-empty shop in the same street during the year of the rent transaction or the previous year if a shop appears to be non-vacant in the previous year and vacant during the year of the transaction.

percent increase in the rent of an average shop. This elasticity is still relatively high compared to the density elasticities found in agglomeration economies literature.

5.4.2 Effects of footfall on vacancy rates

In Table 5.5, we report the results for the incidence of a being vacant using a standard Logit model based on a similar set of specifications as in the previous subsection. The estimated marginal effect is shop-specific, so we report average marginal effects[33]. Column [1] is a naïve regression of a dummy variable indicating if a shop is vacant on log footfall, the log surface area of the building, construction year dummies, location attributes and year fixed effects. The average marginal effect of log footfall is -0.027.

The estimated effect is slightly higher (in absolute value) when we include shopping district or shopping street fixed effects in Columns [2] and [3], respectively. In the last three columns, we focus on our preferred identification strategy where we only include observations close to intersections of shopping streets. In Column [4] we show that if we include fixed effects for shops within 100m from an intersection, the impact of footfall on vacancy rates is very similar. This effect is exactly the same once we reduce the distance bandwidth of the fixed effects to 50m from an intersection in Column [5] and essentially the same when we also include shop and street characteristics in Column [6].

Column [6] is our preferred specification, which suggests that doubling footfall leads to a 1.9 percentage point reduction in vacancies. This reduction is about one third of the average vacancy rate. Thus, the effect of footfall on vacancies is substantial. An increase of one standard deviation in footfall leads to a drop in the vacancy rate of about 1.7 percentage points, roughly a quarter of the average vacancy rate. These results confirm our retail rents results. They suggest that the most attractive locations in terms of footfall have a lower probability to be vacant, in line with the idea that the opportunity cost of having an empty property is higher for the high-rent shops. We test for other explanations in Appendix 5.8.6. For example, we test whether the effect of footfall on vacancy rates is only relevant in times of low demand, when for certain shops the marginal costs of providing shop space are below the marginal benefits.

[33] The marginal effects for the sample averages of the included explanatory variables are very similar to the average marginal effect presented here.

Table 5.5: Regression results for vacant shops

Dependent variable:	dummy shop is vacant					
	[1] OLS	[2] OLS	[3] OLS	[4] OLS	[5] OLS	[6] OLS
Footfall (log)	-0.0278[a]	-0.0288[a]	-0.0311[a]	-0.0277[a]	-0.0279[a]	-0.0276[a]
	(0.00128)	(0.00115)	(0.00151)	(0.00136)	(0.00161)	(0.00163)
Building surface area in m^2 (log)	0.00273[a]	-0.000478	-7.14e-05	-0.000801	0.000842	0.000870
	(0.000929)	(0.000749)	(0.000718)	(0.000748)	(0.000936)	(0.000931)
Property is in mall						0.00612
						(0.00604)
Property on the corner						-0.00542
						(0.00416)
Property is on sunny side of street						-0.000769
						(0.00189)
Shopping street width in m (log)						-0.00726[a]
						(0.00236)
Pedestrian street	0.00898[a]	0.00678[b]	0.0456[a]	0.00470[c]	0.00626[b]	0.00566[c]
	(0.00297)	(0.00266)	(0.00371)	(0.00280)	(0.00290)	(0.00293)
Water within 50m	0.00542	0.0131[a]	0.0134[a]	0.0170[c]	0.0119	0.0119
	(0.00893)	(0.00469)	(0.00512)	(0.00927)	(0.0115)	(0.0113)
Water 50-100m	0.00132	0.00984[a]	0.00906[b]	0.00144	0.00506	0.00493
	(0.00424)	(0.00320)	(0.00399)	(0.00544)	(0.00758)	(0.00762)
In historic district	0.00450[c]	0.00400	0.0118	0.0171[b]	0.0255[b]	0.0235[c]
	(0.00261)	(0.00613)	(0.00733)	(0.00839)	(0.0128)	(0.0127)
Distance to station (log)	-0.00601[a]	-0.00301	-0.000493	0.000561	-0.00196	-0.00209
	(0.00120)	(0.00348)	(0.00456)	(0.00449)	(0.00315)	(0.00315)
Construction year dummies	✓	✓	✓	✓	✓	✓
Year fixed effects	✓	✓	✓	✓	✓	✓
Shopping district fixed effects		✓				
Shopping street fixed effects			✓			
Intersection fixed effects				✓	✓	✓
Log-likelihood	-94305	-92232	-89570	-69467	-44021	-44005
Observations	425,834	425,834	421,204	338,099	220,049	220,049

Notes: Reported coefficients are average marginal effects. Footfall is measured as the number of shoppers per day. In Column [4], we include observations within 100m of a shopping street interaction. In Columns [5] and [6], we reduce this distance to 50m. Robust standard errors clustered at the shopping street level are in parentheses. [a], [b] and [c] indicates significant at 1, 5, and 10 percent level, respectively.

Table 5.6 presents the same specifications as Table 5.5, using as the main variable of interest the log number of shops on the same shopping street as each shop observation, instead of log footfall. The coefficient of the log number of shops is positive and significant in Column [1], which is our most parsimonious specification. In Column [2], where we include shopping district fixed effects, the coefficient of the number of shops becomes negative, albeit not significant. Columns [3] and [4] include 100m and 50m intersection fixed effects, respectively. In both columns, the coefficient of the number of shops in street is negative and statistically significant.

In Column [5], we also add shopping street characteristics. The estimated average marginal effect suggests that doubling the number of shops in an shopping street causes a 0.4 percent decrease in the vacancy rate on average (approximately a 6 percentage point decrease of the average vacancy rate). These results confirm that the number of shops has a direct effect on vacancy rates and highlight the importance of including the effect on vacancy rates in order to derive welfare implications.

Table 5.6: Regression results for vacant shops: Number of shops

Dependent variable:	dummy shop is vacant				
	[1] OLS	[2] OLS	[3] OLS	[4] OLS	[5] OLS
Number of shops in street (log)	0.00464^{b}	-0.000419	-0.00545^{a}	-0.00489^{a}	-0.00527^{a}
	(0.00203)	(0.00107)	(0.00113)	(0.00130)	(0.00129)
Building characteristics	✓	✓	✓	✓	✓
Location characteristics	✓	✓	✓	✓	✓
Shopping street characteristics					✓
Year fixed effects	✓	✓	✓	✓	✓
Shopping district fixed effects		✓			
Intersection fixed effects			✓	✓	✓
Log-likelihood	-94,041	-92,184	-69,460	-44,514	-44,482
Observations	425,783	425,783	338,070	220,020	220,020

Notes: Reported coefficients are average marginal effects. The number of shops in street (log) is the logarithm of the number of shops on the same street and in the same year as each shop observation. Building, location and shopping street characteristics are mentioned in Table 5.5. In Column [3], we include observations within 100m of a shopping street interaction. In Columns [4] and [5], we reduce this distance to 50m. Robust standard errors clustered at the shopping street level are in parentheses. ^{a}, ^{b} and ^{c} indicates significant at 1, 5, and 10 percent level, respectively.

5.5 Counterfactual analysis

5.5.1 Effects of footfall on rental income

In Section 5.2, we argue that shopping externalities are expected to capitalise into rental incomes of shop owners. Rental incomes are defined as the shop rent paid by retail firms multiplied by the share of the time that the shop is occupied. Given the effect of footfall on retail rents and vacancies that we estimated in Sections 5.4.1 and 5.4.2, Table 5.7 provides the estimates for the effect of log footfall on log rental income. Following equation (5.2), we calculate this effect assuming different values of κ:

$$\frac{\partial logI_{ij}}{\partial f_j} = \frac{\partial logp_{ij}}{\partial f_j} - (1+\kappa)\frac{\partial v_{ij}}{\partial f_j} \tag{5.20}$$

κ is a positive parameter that defines the relationship between advertising cost and rental price. We guesstimate κ to be equal to 0.4135, based on the costs of letting commercial space in the Netherlands, which is about 17.5 percent of the yearly rental value (Leurs, 2017)[34]. Table 5.7 reports the estimated effect of footfall on rental income based on the specifications listed in Table 5.3 and Table 5.5.

Table 5.7: Footfall and rental incomes

		[1] OLS	[2] OLS	[3] OLS	[4] OLS	[5] OLS	[6] OLS
Footfall (log)	$\kappa = 0.4135$	0.361^a	0.348^a	0.350^a	0.269^a	0.254^a	0.252^a
		(0.02175)	(0.01815)	(0.03005)	(0.02705)	(0.03495)	(0.03505)
	$\kappa = 0.1181$	0.353^a	0.339^a	0.341^a	0.261^a	0.246^a	0.244^a
		(0.02174)	(0.01814)	(0.03004)	(0.02704)	(0.03494)	(0.03504)
	$\kappa = 0.5907$	0.366^a	0.353^a	0.355^a	0.274^a	0.259^a	0.257^a
		(0.02176)	(0.01816)	(0.03006)	(0.02705)	(0.03496)	(0.03506)
Property characteristics		✓	✓	✓	✓	✓	✓
Building characteristics		✓	✓	✓	✓	✓	✓
Location characteristics		✓	✓	✓	✓	✓	✓
Shop and street characteristics							✓
Year fixed effects		✓	✓	✓	✓	✓	✓
Shopping district fixed effects			✓				
Shopping street fixed effects				✓			
Intersection fixed effects					✓	✓	✓

Notes: Footfall is measured as the number of shoppers per day. Property, building, location and shopping street characteristics are mentioned in Table 5.3. In Column [4], we include observations within 100m of a shopping street interaction. In Columns [5] and [6], we reduce this distance to 50m. Robust standard errors clustered at the shopping street level are in parentheses. ^{a}, ^{b} and ^{c} indicates significant at 1, 5, and 10 percent level, respectively.

Column [1] includes property, building and location characteristics, as well as year fixed effects. The estimated effect of footfall on rental income is between 0.353 and 0.366, depending on the value of κ we assume. Adding shopping district fixed effects in Columns [2] and [3], respectively, has virtually no effect on the estimated coefficient. Columns [4]-[6] report the estimates of our main identification strategy, using intersection fixed effects. In Columns [4] and [5], which include 100m and 50m intersection fixed effects, respectively, the estimated footfall coefficient decreases to 0.269 and 0.254 based on the most realistic value of κ. Finally, when we add shop and street characteristics in Column [6], the elasticity of rental income with respect to footfall is 0.253.

[34]The costs that a property owner incurs to find a new tenant are given by $(costs \times p)/contract/length$, which should be equal to $cv = \kappa pv$. From a small subset of the observation we know that the average contract length is 6.77 years. Furthermore, we know that the vacancy rate is on average 0.0625. Hence, $\kappa = cost/(v \times contract/length) = 0.175/(6.771465 \times 0.0625) = 0.4135$. When fees are, let's say, only 5 percent, $\kappa = 0.118143$, while if fees are 25 percent, $\kappa = 0.590714$

In order to derive the average marginal benefit of an additional average shop, we need to calculate the direct effect of an additional shop on rental income. Table 5.8 follows Table 5.7 using the estimated effects of the number of shops in each shopping street on rents and vacancies[35]. Column [5], which is our more conservative estimate, reports an elasticity of rental income with respect to the number of shops on the street of about 0.107. We will use this elasticity to calculate the average marginal effect of an additional shop in a street in the following section.

Table 5.8: Number of shops and rental income

		[1] OLS	[2] OLS	[3] OLS	[4] OLS	[5] OLS
Footfall (log)	$\kappa = 0.4135$	0.058^a	0.079^a	0.123^a	0.105^a	0.107^a
		(0.02419)	(0.01713)	(0.02233)	(0.02364)	(0.02344)
	$\kappa = 0.1181$	0.059^a	0.079^a	0.121^a	0.104^a	0.106^a
		(0.02419)	(0.01713)	(0.02233)	(0.02364)	(0.02344)
	$\kappa = 0.5907$	0.057^a	0.080^a	0.124^a	0.106^a	0.108^a
		(0.02419)	(0.01713)	(0.02233)	(0.02364)	(0.02344)
Property characteristics		✓	✓	✓	✓	✓
Building characteristics		✓	✓	✓	✓	✓
Location characteristics		✓	✓	✓	✓	✓
Shop and street characteristics						✓
Year fixed effects		✓	✓	✓	✓	✓
Shopping district fixed effects			✓			
Shopping street fixed effects						
Intersection fixed effects				✓	✓	✓

Notes: Footfall is measured as the number of shoppers per day. Property, building, location and shopping street characteristics are mentioned in Table 5.3. In Column [3], we include observations within 100m of a shopping street interaction. In Columns [4] and [5], we reduce this distance to 50m. Robust standard errors clustered at the shopping street level are in parentheses. [a], [b] and [c] indicates significant at 1, 5, and 10 percent level, respectively.

5.5.2 Determining optimal subsidy to shops

We have shown that shopping externalities are important and argued that in a setting as the one we analyse, it is highly unlikely that shopping externalities are capitalised into retail rents. We have demonstrated that the (average) elasticity of rental income with respect to number of shops $\hat{\varepsilon}_{I,N}$ is about 0.107, (see Table 5.8). Using this elasticity, we can calculate the optimal subsidy for a shop, which should be equal to the average marginal *external* benefit of one additional shop.

Hence, according to equation (5.13), the *average* subsidy for retail firms should be about 10 percent of the rental income. Given the different values of κ we used to

[35]Again, the only difference with the estimated specifications in Table 5.7 is that we do not include street fixed effects in Column [3].

calculate the effect of footfall on rental income, the optimal subsidy that should be given to one additional shop is in the range €5,031-5,172.

However, as argued in Section 5.2, shops may be heterogeneous in the amount of footfall they generate for other shops, implying that $\varepsilon_{f,N}$ may be different for different shops. Given equation (5.14) and that $\hat{\varepsilon}_{I,f} = 0.25$, it should hold that the subsidy given to a shop should be larger than 10 percent of the rental income if $\varepsilon_{f,N} > 0.4$. This result suggests that substantial subsidies for certain shops may be welfare improving. For example, suppose that a shopping street consists of hundred small shops, and a large retailer, e.g. a warehouse, considers to leave the shopping street, reducing footfall by 20 percent. Thus, $\varepsilon_{f,N}$ is about 1.2. In this case, it may be efficient to provide subsidies of about 30 percent of the rental expenditure to the store owner. $\varepsilon_{f,N}$ is also interesting if a local government aims to directly increase footfall, for example through subsidised car parking or pedestrianisation of a street.

5.6 Sensitivity analysis

In order to establish the causal relationship between footfall and retail rents, vacancies and therefore rental income, we have estimated alternative specifications to address the main identification concerns that might disparage the validity of our results. We provide here a summary of the main analyses. More details can be found in Appendices 5.8.4-5.8.9.

One first concern with our identification strategy is that our main identification assumption (i.e. that shops located in close proximity from two intersecting streets have similar unobserved characteristics) might not hold. If the two intersecting streets differ in unobserved characteristics that affect both footfall and retail rents, our preferred estimates would be biased. In Appendix 5.8.4, we address this concern by focusing on local differences in footfall between neighbouring shops (within 50m from an intersection) that are located on the *same* shopping street. An alternative way to control for street-specific local endowments is to use fixed effects for the 6-digit postal code (PC6) of each shop. Both these sensitivity checks supports our main identification strategy, suggesting that it is highly unlikely that our estimates suffer from omitted variable bias. As mentioned in Section 5.2.3, another possible concern could be reverse causality. We then use log footfall in the previous year instead. We also address reverse causality concerns by using the average of the logarithm of footfall over the time period instead of the logarithm of annual footfall. Moreover, using the logarithm of the average annual footfall, we mitigate the measurement error in footfall due to the random variation between different Saturdays of each year at the same location. Using lagged footfall, or the average footfall over the study period leads to highly statistically significant effects. The effect is actually slightly higher compared to the baseline results. Another concern we raised is that shops located close to train stations and those located inside a mall may be very different from shops located in ordinary shopping streets. We therefore exclude observation located less than 1km away from a train station and the shops that are considered to be inside a mall, respectively. The estimated coefficients are hardly different from our main estimates. We also run the same general robustness checks for the vacancy analysis and the results are roughly unchanged. Moreover, using a Linear Probability Model instead of a Logit, the estimates of the effect of footfall on vacancy rates are very similar.

In Appendix 5.8.5 we show the results of log footfall on log rents and vacancy rates using quadratic specifications of log footfall to allow for non-linear effects of the logarithm of footfall. These results indicate that $\varepsilon_{I,f}$ is increasing in footfall. Allowing for non-linearity may therefore be important. For example, using a linear specification, we obtain very different effects for central city and suburban shops. Because of large differences in footfall between the city centre and suburbs, this difference turns out to be explained by the non-linear elasticity of footfall with respect to rental income. Moreover, using a non-linear specification, we find that the effect of log footfall on retail rents is identical for pedestrianised and non-pedestrian streets and that pedestrian streets have no direct effect on retail rents. These results are in line with our assumption that footfall captures the economic value of the shop's location.

As mentioned in Section 5.4.2, there might be alternative explanations that explain the effect of footfall on retail vacancy rates. One such explanation is that the effect of footfall on vacancy rates is only relevant in times of low demand, when for certain shops, the marginal costs of providing shop space are below the marginal benefits. On the other hand, in times of high demand, for almost all retail establishments, marginal costs are lower than the marginal benefits, thus, the effect of footfall on vacancy rates could be negligible. We test this hypothesis in Appendix 5.8.6 where we regress the dummy for a vacant shop on the interaction term between log footfall and a dummy variable for the recent boom and bust period of the Dutch economy, respectively. Our results show that the effect of footfall on vacancy rates is statistically different in the boom and bust periods. Specifically, the effect in bust periods is higher, as expected. However, the effect of footfall on vacancy rates is still economically and statistically significant during the boom years. This results confirms that higher rents increase the opportunity cost of having an empty shop, so that vacancy rates are lower in more attractive areas (i.e. those with a higher footfall).

As we mentioned in the introduction, policies that foster retail concentration can be welfare improving only if shopping externalities are not fully internalised. We also argued that the highly fragmented property ownership that we find in our sample implies that internalisation is unlikely to occur[36]. In Appendix 5.8.7, we test whether the effect of footfall on retail rents is capitalised differently in properties that belong to property owners who possess multiple rental properties on the same shopping street (multi-property owners). For multi-property owners, the externality seems to capitalise in rents in the same way as for single property owners. In addition, we do not find any difference between commercial property owners (real estate companies, construction companies etc.) and private property owners. Furthermore, in Appendix 5.8.7 we show that the effect is the same for shops that are part of a retail chain and for non-chain shops.

In Appendix 5.8.8, we test whether the number of shops in a street can fully account for the differential effect of log footfall between high and low footfall intersecting streets. Our results for both rents and vacancies suggest that this is not the case. Therefore, it seems that the potential of shops to generate footfall is quite heterogeneous. In other words, the elasticity of footfall with respect to shops $\varepsilon_{f,N}$ may be very heterogeneous.

In the current paper, we have argued that footfall can be used as a measure of shopping externalities. In the Section 5.4, we have shown that also the number of shops in a shopping street has a meaningful effect on retail rents. In Appendix 5.8.9, we investigate the spatial scope of this question by adding the (log) number of shops that are located on the same street for different distance thresholds (e.g. 100m, 200m) leading to very similar results.

Until now, we have argued that shopping externalities, as measured by footfall, have a substantial positive effect on the retail market. However, the effect of footfall might well extend beyond this market. It has been argued that retail dispersion towards the suburbs may lead to the 'hollowing-out' of city centres, where shops were traditionally concentrated (Sánchez Vidal, 2016). It could thus be argued that footfall may increase the liveability and the attractiveness of city centres. In her magnum opus, Jane Jacobs argues that *"the sidewalk must have users on it fairly continuously, both to add to the number of effective eyes on the street and to induce the people in buildings along the street to watch the sidewalks in sufficient numbers. [...] Large numbers of people entertain themselves, off and on, by watching street activity"* (Jacobs, 1961). On the other hand, we are aware of examples that residents raised opposition to new retail developments next to shopping streets, suggesting that a high retail concentration may also cause negative effects for the residents (e.g. through increased traffic or noise). In Appendix 5.8.10, we test for the existence of

[36] In our sample, only 18 percent of properties belong to owners who possess multiple properties on the same street.

such external effects by analysing the effect of footfall on residential housing prices and we find no effect of the ’people on the street’. This result suggests that the net effect of footfall externalities on residents is zero.

5.7 Conclusions

The findings of this paper add to our understanding of retail agglomeration and shopping externalities. We have built a theoretical framework to introduce and explain the effect of footfall on retail rents and vacancies. Our model suggests that *(i)* shop rents depend *positively* on footfall and *(ii)* the number of vacancies depends *negatively* on footfall. Hence, the effect of footfall on rental income, i.e. the shop rent multiplied with the share of the time that the shop is occupied, is positive.

Our empirical estimates show that the effect of footfall on retail rents and vacancies is substantial. We estimated an elasticity of rental income with respect to rental income of approximately 0.25. Both this elasticity and the elasticity of rental income with respect to the number of shops (0.1) are considerably higher than the standard estimates in the empirical literature of agglomeration economies. Therefore, shopping externalities seem to be crucial to the retail location choices. Our results are very robust to different identification strategies including the use of very local variation in footfall and an extensive set of control variables.

Our analysis highlights the fundamental heterogeneity of shops in their ability to attract customers to shopping streets and therefore, to generate positive shopping externalities for other shops. We show that employing the number of shops in the neighbourhood of a shop rather than footfall generates a strong downward bias of the externality we are interested in. In addition, while shopping malls have been extensively studied in the literature, we analyse shopping streets, which are much more common in Europe’s cities than in US. We show that the fragmented property ownership in Dutch shopping streets means that shopping externalities are not internalised in the market and thus, welfare is not maximised.

In order to formulate our policy recommendations, we derive an average optimal subsidy per pedestrian visiting a shop, as well as a subsidy *per shop* that could be paid to a retail firm as an incentive to establish a shop in a shopping street with a considerable number of shops. The optimal annual subsidy, which equals the average marginal external benefit of an additional shop, is about €5,000, but should be higher if shops generate more footfall for surrounding shops.

The implications of our findings contribute to a heated policy debate on the ’hollowing-out’ of city centres in some European countries and the rise of large ’big-box’ stores near the urban fringe. It is also complementary to a literature which demonstrates that the welfare effects of current planning policies that *hinder* entry in retail markets, and particularly of large retailers, are negative. However, the use of the number of people accessing the shopping street on foot has also important implication for transport policies. Alternative policies could subsidise public transportation or parking spaces to facilitate the accessibility to these shopping streets or to improve the attractiveness of these streets for pedestrians, for example, by pedestrianising them or by offering complementary activities (e.g. museums and galleries).

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5.8 Appendix

5.8.1 Proof of Proposition 5.1

We first derive V, R, S and p by solving the system of equations (5.4), (5.5), (5.6) and (5.8). This leads to:

$$V = \frac{(1-\beta)mf - (r+\delta)cv}{r((1-\beta)m + (r+\delta)v)} \tag{5.21}$$

$$R = \frac{f(m+rv)(1-\beta) - (r\beta+\delta)cv}{r((1-\beta)m + v(r+\delta))} \tag{5.22}$$

$$S = \frac{(c+f)v\beta}{m(1-\beta) + v(r+\delta)} \tag{5.23}$$

$$p = \frac{f(1-\beta(v(r+\delta)+m) - (r+\delta)v\beta c}{(1-\beta)m + v(r+\delta)} \tag{5.24}$$

Note that $m = m(ev)$ and $c = c(e)$.

First, we are interested in the effect of footfall on rents, so dp/df. We then use equations (5.9), (5.10) and (5.11), and using implicit differentiation. According to Cramer's rule, $dp/df = det(Z_p)/det(Z)$, where:

$$Z = \begin{pmatrix} 1 & \frac{(1-v)(1-\beta)(r+\delta)(f+c)\beta\delta v}{e(rv+\delta(1-\beta(1-v)))^2} & 0 \\ 0 & \frac{m}{\delta e} & 1+\frac{m}{\delta v} \\ 0 & -\frac{(1-\beta)mc'}{erv+e\delta(1-\beta(1-v))} + c'' & -\frac{(1-\beta)^2(f+c)\delta m}{ev(rv+\delta(1-\beta(1-v)))^2} \end{pmatrix} \tag{5.25}$$

Note that $c' = \partial c/\partial e$ and $c'' = \partial^2 c/\partial e^2$. To obtain Z_p we replace the first column of Z with:

$$z = \begin{pmatrix} \frac{(1-\beta)(rv+\delta)}{rv+\delta(1-\beta(1-v))} \\ 0 \\ \frac{(1-\beta)m(e)}{erv+e\delta(1-\beta(1-v))} \end{pmatrix} \tag{5.26}$$

To obtain $det(Z_e)$ and $det(Z_v)$, we replace respectively the second and third column of Z with z. We then take into account that $m = \delta(1-v)$ and use (5.11) to obtain:

$$\frac{dp}{df} = \frac{det(Z_p)}{det(Z)} = \frac{(1-\beta)(rv+\delta)}{\Delta} > 0 \tag{5.27}$$

$$\frac{de}{df} = \frac{det(Z_e)}{det(Z)} = \frac{(1-v)(1-\beta)\delta}{\Delta ec''} > 0 \tag{5.28}$$

$$\frac{dv}{df} = \frac{det(Z_v)}{det(Z)} = -\frac{(1-v)^2(1-\beta)\delta v}{\Delta e^2 c''} < 0 \tag{5.29}$$

where $\Delta = rv + \delta(1-\beta(1-v))$. Because $\beta < 1$, the impact of footfall on rents is positive, $dp/df > 0$. Furthermore, because the cost function is convex, we have $c'' > 0$, so that $de/df > 0$. This implies that advertising expenditure will increase when footfall is higher. Consequently, when advertising expenditure increases, the matching rate will also increase implying that $dv/df < 0$ (see equation (5.10)), which is confirmed by equation (5.29).

5.8.2 Proof of Proposition 5.2

Using implicit differentiation and Cramer's rule, we establish that:

$$\frac{dv}{d\bar{f}} = -\frac{\delta v(1-v)^3(1-\beta)}{\Delta e^2 c'' - (1-v)^2(1-\beta)\delta v\bar{f}} \tag{5.30}$$

We also obtain the second derivative with respect to $\bar{f}$:

$$\frac{dv^2}{d^2\bar{f}} = -\frac{(1-v)^5 v^2 (1-\beta)^2\delta^2}{(e^2\Delta c'' - (1-v)^2(1-\beta)\delta v\bar{f})^2} < 0 \tag{5.31}$$

Using implicit differentiation, it should hold that:

$$\frac{dp}{df} = \frac{dp}{d\bar{f}} \Big/ \frac{df}{d\bar{f}} \qquad \text{and} \qquad \frac{dv}{df} = \frac{dv}{d\bar{f}} \Big/ \frac{df}{d\bar{f}} \tag{5.32}$$

So if $df/d\bar{f} > 1$, it holds that $dp/d\bar{f} > dp/df$ and $dv/d\bar{f} < dv/d\bar{f}$. Hence:

$$\frac{df}{d\bar{f}} = -\frac{dv}{d\bar{f}}\bar{f} + (1-v) > 1 \tag{5.33}$$

implying that $-dv/d\bar{f} > v/\bar{f}$. Because $dv^2/d^2f < 0$, this condition holds.

5.8.3 Data appendix

Our main analysis only requires information on footfall, which is obtained from the Locatus dataset using a one-to-one shop matching based on location. However, for some sensitivity analyses we need information on the type of retail firm that is occupying a shop. We therefore use a matching process to obtain the retail branch code for each shop and whether the shop is a part of a chain or not. It should be mentioned that our results are not sensitive to this matching process.

The sectoral classification of the shops in the Strabo dataset is not very detailed. By contrast, the Locatus data provide information on several shop attributes (shop name, address and whether the shop is part of a chain) and includes a detailed sectoral classification up to the branch level (e.g. it distinguishes between male and female clothing, shoes etc.). The matching process between the Strabo and Locatus datasets is based on shops that are in the Locatus dataset the same year or up to two years after the rental transaction (the two-year period seems reasonable since a retail firm usually keeps the shop vacant to refurbish the establishment after renting a property).[37]

The most accurate way for matching is to use the exact shop name and building id. In this way, we matched 5.64 percent of the Strabo data. Although the shop coordinates in the two datasets are very accurate, in some cases shops might be matched to another building close by. Using the exact names, street number and the 6-digit postal codes (PC6), we matched 19.37 percent additional shops. In the Netherlands, the combination of each PC6 and each street number is unique. However, several observations have missing street numbers. On account of this fact, in a further step we use only the exact names and the PC6 codes and we match a further 1.93 percent of our sample. Hence, in total we matched 26.94 percent of the Strabo data using the complete name of each shop combined with some other exact location criteria.

Frequently, the name of the same shop does not appear identical in the two datasets. For this reason, we use the two first characters of the names in the two datasets for the rest of the shops (excluding articles such as "the" and other common words). Hence, using building id's, then PC6 and building numbers and finally, PC6, together with the first two letters of the shop names in the two datasets, we merged a further 5.64, 25.34 and 7 percent, respectively, adding up to a cumulative 64.92 percent of the shops in our sample.

The rest of the shops were matched based on the whole name and the 4-digit postal code (PC4), which approximately corresponds to a building block. This way we matched a further 3.19 percent of our sample and the remaining 31.88 percent was matched based on the exact names alone. We emphasise that we have double-checked manually all the matched observations and the results are very accurate.

5.8.4 Sensitivity analysis - general robustness checks

Here, we present additional results confirming the results obtained in in Section 5.4. We start by analysing the effects of footfall on rents. Our baseline specification is

[37] Although Locatus gathers information on shops during the whole year.

reported in Column [6] of Table 5.3. In that specification, we regress log rents on log footfall controlling for property, building and location characteristics, construction year dummies, year fixed effects, shopping street and other shop characteristics, as well as 50m intersection fixed effects based on the distance of each shop to its closest street intersection. The coefficient of log footfall we estimated is 0.213 and highly statistically significant.

Table 5.9: Robustness checks for retail rents

Dependent variable:	log(rent)					
	[1]	[2]	[3]	[4]	[5]	[6]
	OLS	OLS	OLS	OLS	OLS	OLS
Footfall (log)	0.136^a	0.127^a			0.232^a	0.223^a
	(0.0474)	(0.0379)			(0.0346)	(0.0674)
Footfall (log) one-year lag			0.233^a			
			(0.0339)			
Footfall (log) annual average				0.272^a		
				(0.0398)		
Property characteristics	✓	✓	✓	✓	✓	✓
Building characteristics	✓	✓	✓	✓	✓	✓
Location characteristics	✓	✓	✓	✓	✓	✓
Shopping street characteristics	✓	✓	✓			✓
Year fixed effects	✓	✓	✓	✓	✓	✓
PC6 fixed effects		✓				
Shopping street fixed effects	✓					
Intersection fixed effects	✓		✓	✓	✓	✓
Shopping street×intersection fixed effects	✓					
Observations	1,471	1,373	1,839	1,870	1,793	425
R^2	0.888	0.887	0.873	0.874	0.873	0.904

Notes: The coefficients of log footfall using our preferred specification (Column (6) in Table 5.3) and the same sample as in Columns [1] and [2], Table 5.9 are 0.193^a(0.039) and 0.218^a(0.048), respectively. Footfall is measured as the number of shoppers per day. Property characteristics are the size of property in m^2 (log), dummy variables for new and renovated buildings, as well for sublet properties. Building characteristics are building surface area in m^2 (log) and construction year dummies. Location characteristics are dummies for pedestrian streets, for proximity to water within 50m, or in the range 50-100m, for historic districts and for the distance to the closest station (log). Shop and street characteristics are dummies for properties in malls, on corners, on the sunny side of street, as well as the shopping street width in m (log). In Columns [1]-[6], we include observations within 50m of a shopping street interaction. In Column [5], we have excluded the shops inside a mall and in Column [6], we have excluded the shops located further than 1 km from the closest train station. Robust standard errors clustered at the shopping street level are in parentheses. ^{a}, ^{b} and ^{c} indicates significant at 1, 5, and 10 percent level, respectively.

In Column [1] of Table 5.9, we use instead of 50m intersection fixed effects, street-specific 50m intersection fixed effects (interacting the shopping street dummy with the 50m intersection dummy). Consequently, we control for time invariant local endowments that are the same in all shops located on the same street where the distance between these shops is less than 100m. The magnitude of the estimated elasticity is lower (0.136) since these fixed effects absorb a considerable part of the identifying variation (between intersecting streets)[38]. However, the effect is still highly statistically significant.

This result suggests that our findings are robust even when we control for unobservable endowments that are the same between shops located in close proximity and in the same shopping street. Another way to control for street-specific local endowments is to use fixed effects for the 6-digit postal code (PC6) of each shop. PC6 refers to roughly one side of a building block, approximating the street-specific 50m intersection interaction fixed effect. The main difference between the two fixed effects is that PC6 is an administrative area. Column [2] of Table 5.9 includes PC6 fixed effects. The estimated log footfall effect is 0.127 and highly statistically significant[39].

We also estimate our preferred specification using one or two-year lags of footfall to control for reverse causality. Column [3] reports the log footfall coefficient in the previous year than the rent transaction took place. The estimated coefficient is still highly statistically significant and although we lose some observations from our sample, its value is 0.233, very similar to our preferred estimate. Using a two-year lag of log footfall (not reported in Table 5.9), our estimated effect is 0.243 and highly statistically significant[40].

The third concern of measurement error in footfall that we discussed in Section 5.3.1 is related to random variation between different Saturdays of each year at the same location. Although our identification strategy is based on spatial differences in footfall and rents, in Column [4] we use the annual average of the logarithm of footfall in order to fully address this concern. This measure of footfall is also robust to reverse causality. The estimated coefficient is 0.272 and highly statistically significant. Again, the coefficient is higher but not statistically different from our main estimate. Another concern we raised is that shops located close to train stations and those located inside a mall may be very different from shops located in ordinary shopping streets. For this reason, in Columns [5] and [6], we exclude observations located less than 1km away from a train station and the shops that are inside a mall, respectively. As we can see in Table 5.9, the estimated coefficients are hardly different from our main estimates.

We run the same set of robustness checks for vacancies in Table 5.10. The reported results are estimated using a Logit regression based on our preferred specifi-

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[38] The coefficient of log footfall that we obtain using our baseline specification (Column (6)) in Table 5.3 with the same sample as in column [1], Table 5.9, is 0.193 and highly statistically significant.

[39] Again, if we regress our baseline specification with the same sample as in column [2], Table 5.9, is 0.218 and highly statistically significant.

[40] Again, if we regress our baseline specification with the same sample as in Column [2], Table 5.9, the log footfall coefficient is 0.218 and highly statistically significant.

cation used in Column [6], Table 5.5.

In Column [1] of Table 5.10, we compare shops located less than 50m away from an intersection on the same street by including street×intersection fixed effects. The reported coefficient is -0.0290, essentially the same as in in our baseline specification. Column [2] in Table 5.10 is different from Column [2] in Table 5.9, where we use PC6 fixed effects. Given that we can observe each shop on an annual basis, we use retail establishment (shop) fixed effects and we obtain identification only based on temporal variation of vacancies and footfall. The fact that our results are still highly statistically significant and in the same order of magnitude as in our main estimates is reassuring.

Table 5.10: Robustness checks for vacant shops

Dependent variable:	dummy shop is vacant					
	[1] OLS	[2] OLS	[3] OLS	[4] OLS	[5] OLS	[6] OLS
Footfall (log)	-0.0290^a (0.00217)	-0.0494^a (0.00799)			-0.0266^a (0.00158)	-0.0272^a (0.00247)
Footfall (log) one-year lag			-0.0254^a (0.00163)			
Footfall (log) annual average				-0.0313^a (0.00201)		
Building characteristics	✓	✓	✓	✓	✓	✓
Location characteristics	✓	✓	✓	✓	✓	✓
Shopping street characteristics	✓	✓	✓			✓
Year fixed effects	✓	✓	✓	✓	✓	✓
Shop fixed effects		✓				
Shopping street fixed effects	✓					
Intersection fixed effects	✓		✓	✓	✓	✓
Shopping street?intersection fixed effects	✓					
Log-likelihood	200,377	66,940	194,808	220,049	206,117	57,595
Observations	-42,010	-27,246	-36,870	-44,042	-41,540	-11,102

Notes: Reported coefficients are average marginal effects. Footfall is measured as the number of shoppers per day. Building characteristics are building surface area in m^2 (log) and construction year dummies. Location characteristics are dummies for pedestrian streets, for proximity to water within 50m, or in the range 50-100m, for historic districts and for the distance to the closest station (log). Shop and street characteristics are dummies for properties in malls, on corners, on the sunny side of street, as well as the shopping street width in m (log). In Columns [1]-[6], we include observations within 50m of a shopping street interaction. In Column [5], we have excluded the shops inside a mall and in Column [6], we have excluded the shops further than 1 km from the closest train station. Robust standard errors clustered at the shopping street level are in parentheses. ^{a}, ^{b} and ^{c} indicates significant at 1, 5, and 10 percent level, respectively.

Columns [3] and [4] in Table 5.10 include a one-year lag of log footfall and the annual average of log footfall, respectively, instead of the yearly footfall as the main variable of interest. In line with the robustness checks for the rent analysis (Table 5.9), the coefficient of lag footfall (log) is slightly lower while the coefficient of the annual average of log footfall is slightly higher (in absolute terms) than the estimates using the annual log footfall. These results confirm that any bias introduced by reverse causality or by measurement error in annual footfall is not substantial. Finally, in Columns [5] and [6], Table 5.10, we exclude shops that are inside a mall and shops that are close to a train station, respectively. The coefficient of log footfall is not significantly different from our main results, confirming that such shops do not drive our main results.

Table 5.11: Regression results for vacant shops: Linear probability model

Dependent variable:	dummy shop is vacant					
	[1]	[2]	[3]	[4]	[5]	[6]
	OLS	OLS	OLS	OLS	OLS	OLS
Footfall (log)	-0.0309^a	-0.0325^a	-0.0354^a	-0.0315^a	-0.0312^a	-0.0309^a
	(0.00147)	(0.00143)	(0.00200)	(0.00183)	(0.00229)	(0.00232)
Building surface area in m^2 (log)	0.00275^a	-0.000473	4.04e-05	-0.000569	0.00102	0.00108
	(0.000956)	(0.000777)	(0.000747)	(0.000763)	(0.000927)	(0.000924)
Property is in mall						0.00644
						(0.00666)
Property on the corner						-0.00490
						(0.00375)
Property is on sunny side of street						-0.000924
						(0.00187)
Shopping street width in m (log)						-0.00630^a
						(0.00236)
Pedestrian street	0.00941^a	0.00762^a		0.00570^b	0.00665^b	0.00616^b
	(0.00307)	(0.00277)		(0.00287)	(0.00305)	(0.00307)
Water within 50m	0.00655	0.0141^a	0.0117^b	0.0177^c	0.0102	0.0102
	(0.00929)	(0.00534)	(0.00537)	(0.0107)	(0.0117)	(0.0116)
Water 50-100m	0.00121	0.00900^a	0.00752^c	0.000925	0.00383	0.00369
	(0.00424)	(0.00339)	(0.00401)	(0.00542)	(0.00703)	(0.00704)
In historic district	0.00391	0.00932	0.0114	0.0160^c	0.0264^c	0.0246
	(0.00255)	(0.00812)	(0.00808)	(0.00921)	(0.0156)	(0.0155)
Distance to station (log)	-0.00632^a	-0.00462	-0.00426	-0.00364	-0.00748	-0.00769
	(0.00115)	(0.00378)	(0.00552)	(0.00680)	(0.00581)	(0.00582)
Construction year dummies	✓	✓	✓	✓	✓	✓
Year fixed effects	✓	✓	✓	✓	✓	✓
Shopping district fixed effects		✓				
Shopping street fixed effects			✓			
Intersection fixed effects				✓	✓	✓
Observations	425,834	425,834	425,834	338,070	220,020	220,020
R^2	0.018	0.028	0.043	0.046	0.052	0.052

Notes: Footfall is measured as the number of shoppers per day. In Column [4], we include observations within 100m of a shopping street interaction. In Columns [5] and [6], we reduce this distance to 50m. Robust standard errors clustered at the shopping street level are in parentheses. ^{a}, ^{b} and ^{c} indicates significant at 1, 5, and 10 percent level, respectively.

Table 5.11 reports the results of Table 5.5 using a Linear Probability Model

(LPM) instead of a logistic regression. The reported coefficients can be directly interpreted as semi-elasticities. These results are very similar to the average marginal effects that we discussed in Section 5.4.2, Column [1] is a naïve specification, which only includes building and location characteristics, construction year dummies and year fixed effects. The coefficient in Column [1] suggests that doubling footfall leads to a 2.1 percentage point reduction in vacancies. When we include shopping district fixed effects in Column [2], street fixed effects in Column [3] or 100m intersection fixed effects in Column [4], the effect is essentially the same. Column [5], which includes 50m intersection fixed effects and Column [6], which additionally includes street and shop characteristics, yields the exact same coefficient as in Column [1], confirming our main results.

5.8.5 Sensitivity analysis - nonlinearity

We discuss here the results given quadratic specifications in order to allow for a non-linear effect of the logarithm of footfall on the logarithm of rents. We demean the logarithm of footfall by subtracting the logarithm of average footfall of the whole sample from each observation. In Table 5.12, we show the results of all our main rent specifications (shown in Table 5.3) using the demeaned log footfall and its square, instead of the annual log footfall. In Column [1], we estimated a parsimonious specification where we control for property, building and location characteristics and year fixed effects. We observe that the square term is statistically significant and positive, while the coefficient of log footfall is also highly statistically significant and considerably higher than in our main results (0.322). In Columns [2], [3] and [4], we add shopping district, shopping street and 100m shopping intersection fixed effects, respectively. From Columns [1]-[4], the coefficient of log footfall drops from 0.438 to 0.302. Adding 50m shopping street intersection fixed effects in Column [5], as well as shop and street characteristics in Column [6], reduces the coefficient of log footfall to 0.285 and 0.283, respectively. These results indicate that the elasticity of rents with respect to footfall is increasing in footfall.

Table 5.13 presents a quadratic specification for the vacancy analysis using a logistic regression. The main independent variables are the demeaned log footfall and its square and the reported coefficients are average marginal effects. Again, we start from a naïve specification in Column [1], which includes building and location characteristics and year fixed effects. In Columns [2]-[6], we gradually add shopping district, shopping street, 100m, 50m shopping intersection fixed effects and shop and street characteristics, respectively. In all specifications of Table 5.13, the squared term is statistically significant. The coefficient of the demeaned footfall in Column [6], Table 5.13, is -0.0367, which is substantially higher than the coefficient of log footfall in Table 5.5 (-0.0272) [41].

Table 5.12: Polynomial regression results for retail rents

Dependent variable:	log(rent)					
	[1] OLS	[2] OLS	[3] OLS	[4] OLS	[5] OLS	[6] OLS
Footfall (log) - average Footfall (log)	0.438^a	0.396^a	0.376^a	0.302^a	0.285^a	0.283^a
	(0.0300)	(0.0230)	(0.0332)	(0.0302)	(0.0362)	(0.0362)
(Footfall (log) - average Footfall (log))^2	0.0990^a	0.0759^a	0.0726^a	0.0615^a	0.0692^a	0.0690^a
	(0.0139)	(0.0108)	(0.0155)	(0.0124)	(0.0142)	(0.0143)
Property characteristics	✓	✓	✓	✓	✓	✓
Building characteristics	✓	✓	✓	✓	✓	✓
Construction year dummies	✓	✓	✓	✓	✓	✓
Year fixed effects	✓	✓	✓	✓	✓	✓
Shopping district fixed effects		✓				
Shopping street fixed effects			✓			
Intersection fixed effects				✓	✓	✓
Observations	3,102	3,102	3,102	2,629	1,870	1,870
R^2	0.606	0.723	0.814	0.851	0.875	0.875

Notes: Footfall is measured as the number of shoppers per day. Property, building, location and shopping street characteristics are mentioned in Table 5.9. In Column [4], we include observations within 100m of a shopping street interaction. In Columns [5] and [6], we reduce this distance to 50m. Robust standard errors clustered at the shopping street level are in parentheses. ^{a}, ^{b} and ^{c} indicates significant at 1, 5, and 10 percent level, respectively.

One important street characteristic that we control for in all our specifications is a dummy for whether a street is pedestrianised. In our dataset, about 80 percent of shops are located in pedestrian streets. Not surprisingly, mean footfall is about twice as high in pedestrian streets. It is important to point out that the dummy variable for pedestrian streets will be endogenous when the model is misspecified (because of the strong positive correlation between pedestrian streets and footfall). In Table 5.14, we have estimated a model interacting the pedestrianised and non-pedestrian streets with the demeaned log footfall and its square, the same transformed variables we used in Table 5.12.

[41] It should be mentioned that we use a non-linear specification to test whether the estimated effect is robust to such a specification. However, the focus of the paper is on the average effect. Thus, we use the linear log specification in the main results of the paper.

Table 5.13: Polynomial regression results for vacant shops

Dependent variable:	dummy shop is vacant					
	[1]	[2]	[3]	[4]	[5]	[6]
	OLS	OLS	OLS	OLS	OLS	OLS
Footfall (log) - average Footfall (log)	-0.0432[a]	-0.0435[a]	-0.0434[a]	-0.0398[a]	-0.0370[a]	-0.0367[a]
	(0.00237)	(0.00176)	(0.00189)	(0.00201)	(0.00250)	(0.00252)
(Footfall (log) - average Footfall (log))^2	-0.00767[a]	-0.00723[a]	-0.00610[a]	-0.00587[a]	-0.00445[a]	-0.00442[a]
	(0.000907)	(0.000776)	(0.000874)	(0.00100)	(0.00122)	(0.00123)
Construction year dummies	✓	✓	✓	✓	✓	✓
Year fixed effects	✓	✓	✓	✓	✓	✓
Shopping district fixed effects		✓				
Shopping street fixed effects			✓			
Intersection fixed effects				✓	✓	✓
Log-likelihood	-93,914	-91,926	-89,420	-69,358	-43,983	-43,967
Observations	425,834	425,834	421,204	338,099	220,049	220,049

Notes: Reported coefficients are average marginal effects. Footfall is measured as the number of shoppers per day. Building, location and shopping street characteristics are mentioned in Table 5.10. In Column [4], we include observations within 100m of a shopping street interaction. In Columns [5] and [6], we reduce this distance to 50m. Robust standard errors clustered at the shopping street level are in parentheses. [a], [b] and [c] indicates significant at 1, 5, and 10 percent level, respectively.

Column [1], Table 5.14, which is a naïve specification that includes property, building and location characteristics, together with year fixed effects, shows that the coefficients related to pedestrian streets and non-pedestrian streets seem to differ. However, this difference becomes statistically insignificant when we include shopping district, street and 100m intersection fixed effects in Columns [2], [3] and [4], respectively. Finally, in Column [5], where we include 50m intersection fixed effects, and in Column [6], where we also add shop and street characteristics, we find that both the linear and the quadratic terms for pedestrian and non-pedestrian streets are identical and that the pedestrian street dummy is equal to zero (this holds for the individual constraints, as well as for a F-test, which jointly tests the three constraints). These results are in line with our assumption that the logarithm of footfall fully captures shopping externalities (otherwise the dummy for pedestrian streets would be greater than zero).

Table 5.14: Polynomial regression results for retail rents: Pedestrian streets

Dependent variable:	log(rent)					
	[1] OLS	[2] OLS	[3] OLS	[4] OLS	[5] OLS	[6] OLS
Pedestrian×footfall (log demeaned)	0.457^a	0.404^a	0.380^a	0.292^a	0.283^a	0.281^a
	(0.0327)	(0.0247)	(0.0356)	(0.0309)	(0.0377)	(0.0378)
Pedestrian×footfall (log demeaned)^2	0.0986^a	0.0655^a	0.0610^a	0.0429^b	0.0562^b	0.0557^b
	(0.0176)	(0.0133)	(0.0220)	(0.0197)	(0.0264)	(0.0267)
Non-pedestrian×footfall (log demeaned)	0.296^a	0.326^a	0.326^a	0.343^a	0.278^a	0.282^a
	(0.0485)	(0.0456)	(0.0704)	(0.0897)	(0.0906)	(0.0911)
Non-pedestrian×footfall (log demeaned)^2	0.0555^a	0.0663^a	0.0704^a	0.0827^a	0.0741^a	0.0755^a
	(0.0191)	(0.0170)	(0.0203)	(0.0261)	(0.0240)	(0.0240)
Property characteristics	✓	✓	✓	✓	✓	✓
Building characteristics	✓	✓	✓	✓	✓	✓
Location characteristics	✓	✓	✓	✓	✓	✓
Shopping street characteristics						✓
Year fixed effects	✓	✓	✓	✓	✓	✓
Shopping district fixed effects		✓				
Shopping street fixed effects			✓			
Intersection fixed effects				✓	✓	✓
Log-likelihood	-93,898	-91,906	-89,402	-69,338	-43,957	-43,942
Observations	425,834	425,834	421,204	338,099	220,049	220,049

Notes: Footfall (log demeaned) is calculated by subtracting the log of the annual mean of footfall from footfall (log). Footfall is measured as the number of shoppers per day. Property, building, location and shopping street characteristics are mentioned in Table 5.9. In Column [4], we include observations within 100m of a shopping street interaction. In Columns [5] and [6], we reduce this distance to 50m. Robust standard errors clustered at the shopping street level are in parentheses. ^{a}, ^{b} and ^{c} indicates significant at 1, 5, and 10 percent level, respectively.

5.8.6 Sensitivity analysis - booms and busts

Here, we consider an alternative explanation for the negative effect of footfall on vacancies. In times of high demand, the marginal costs of providing shop space are likely to be above the marginal benefits for most of the shops, so footfall might not have a statistically significant effect on vacancy rates during a boom period. However, in bust times because marginal costs of providing space may be above the marginal benefits, retail space may lie empty in areas with lower rents (i.e. with lower footfall). Hence, footfall may only have an effect during busts. We test this hypothesis by regressing a dummy for a vacant shop on the interaction term between log footfall and a dummy for the recent boom (2003-2008) and bust (2009-2015) period of the Dutch economy, respectively[42]. Table 5.15 reports the results.

[42]The actual years of recession were 2009, 2012, 2013 and 2015. We have also performed the same exercise using the exact years that the economy was in recession. The results are virtually the same.

Table 5.15: Regressions results for vacant shops: booms and busts

Dependent variable:	dummy shop is vacant					
	[1]	[2]	[3]	[4]	[5]	[6]
	OLS	OLS	OLS	OLS	OLS	OLS
Boom×footfall (log)	-0.0259[a]	-0.0259[a]	-0.0287[a]	-0.0254[a]	-0.0243[a]	-0.0241[a]
	(0.00139)	(0.00127)	(0.00159)	(0.00149)	(0.00183)	(0.00185)
Bust×footfall (log)	-0.0288[a]	-0.0303[a]	-0.0323[a]	-0.0289[a]	-0.0296[a]	-0.0294[a]
	(0.00139)	(0.00127)	(0.00164)	(0.00153)	(0.00180)	(0.00182)
Building characteristics	✓	✓	✓	✓	✓	✓
Location characteristics	✓	✓	✓	✓	✓	✓
Shopping street characteristics						✓
Year fixed effects	✓	✓	✓	✓	✓	✓
Shopping district fixed effects		✓				
Shopping street fixed effects			✓			
Intersection fixed effects				✓	✓	✓
Log-likelihood	-94,298	-92,216	-89,560	-69,459	-44,010	-43,994
Observations	425,834	425,834	421,204	338,099	220,049	220,049

Notes: Reported coefficients are average marginal effects. Footfall is measured as the number of shoppers per day. The boom period is 2003-2008 and the bust period is 2009-2015. Footfall is measured as the number of shoppers per day. Building, location and shopping street characteristics are mentioned in Table 5.10. In Column [4], we include observations within 100m of a shopping street interaction. In Columns [5] and [6], we reduce this distance to 50m. Robust standard errors clustered at the shopping street level are in parentheses. ^{a}, ^{b} and ^{c} indicates significant at 1, 5, and 10 percent level, respectively.

Column [1], Table 5.15 includes only building, location characteristics and year fixed effects, as control variables. The coefficient of log footfall in the boom and the bust period is -0.0259 and -0.0288, respectively, and they are both highly statistically significant. While these two coefficients are not statistically different, when we include shopping district fixed effects in Column [2], this difference becomes significant. Including street fixed effects in Column [3] or fixed effects for observations within 100m from their closest intersection in Column [4], makes the difference between the log footfall coefficient for the boom and the bust period non-statistically significant. Finally, in Column [5], where we restrict the sample to observations within 50m from an intersection, and in Column [6], where we also add shop and street characteristics, the difference between the effect of shopping externalities on vacancies between the boom and the bust period becomes statistically significant again. The difference between the two coefficients corroborates our intuition that in times of low demand, an increase in footfall raises marginal benefits above marginal costs for certain shops. Nevertheless, in times of high demand, the effect of footfall on vacancies is in line with the opportunity cost hypothesis; property owners' opportunity cost of not filling a vacant shop increases with footfall. Overall, these results suggest that the effect of footfall on vacancy rates in times of high demand is lower but still highly statistically significant.

5.8.7 Sensitivity analysis - retail chains and property ownership

Another potential concern is that the location decisions of independent retailers and shops that are part of a retail chain could be fundamentally different. Chains may be interested in maximizing their profits 'globally' while independent retailers only focus on the local market. If this hypothesis holds, then chains would engage in coordinated strategic location choices in order to maximize their total catchment area and they would avoid unnecessary local competition between their shops. An alternative strategy for a retail chain could be to establish various shops in close proximity to deter the entrance of possible competitors in the market[43]. Furthermore, advertising may also influence the location choices of chain shops. On the one hand, exposure to high footfall may be good advertisement that may yield popularity for the whole retail chain. On the other hand, due to the advertising campaigns of the big retail chains, the probability that a pedestrian passing by enters a shop and purchases something could be higher for chains compared to independent retail firms. Table 5.16 sheds light into this issue and reports the results when we split our sample into chain shops and non-chain shops.

Again, we follow the specifications used in Table 5.3. Column [1] in Table 5.16, shows the results of the naïve specification where we control for property, building and location characteristics, as well as for year fixed effects. In Columns [2], [3] and [4], we use shopping district, street and 100m intersection fixed effects, respectively. While the coefficients for chain shops and non-chain shops appear to be different in Columns [1] and [4], they are not statistically different. Moreover, when we include 50m intersection fixed effects in Column [5] and shop and street characteristics in Column [6], the estimated coefficients of log footfall for chain shops and non-chain shops are very similar. These results suggest that both chains and non-chains value shopping externalities similarly. Thus, it seems unlikely that their behaviour regarding their localization is fundamentally different.

As we mentioned in the introduction, policy intervention fostering the concentration of footfall-generating retail activities can be welfare improving only if the external effect of footfall is not internalised. In the introduction we argued that internalisation is unlikely to occur in the Netherlands due to the fragmentation of property ownership. As an empirical test for this argument, we use the information of property owner name and property owner type, which is available in the Strabo property dataset in order to test whether different ownership statuses yield different

[43]In reality, some big chains tend to locate many of their shops in close proximity, even within the same shopping street.

estimates of the effect of footfall on retail rents. As mentioned in Section 5.3.3, information on property owner name is available for about one third of the sample that we use in the rent analysis while information on property owner type is available for about two thirds of the same sample.

Table 5.16: Regressions results for retail rents: chains and non-chains

Dependent variable:	log(rent)					
	[1]	[2]	[3]	[4]	[5]	[6]
	OLS	OLS	OLS	OLS	OLS	OLS
	Panel A: Chains					
Footfall (log)	0.320^a	0.266^a	0.242^a	0.141^a	0.156^a	0.162^a
	(0.0318)	(0.0311)	(0.0539)	(0.0529)	(0.0603)	(0.0623)
Observations	1,118	1,118	1,118	984	715	715
R^2	0.602	0.729	0.848	0.913	0.934	0.934
	Panel B: Non-chains					
Footfall (log)	0.277^a	0.266^a	0.274^a	0.193^a	0.176^a	0.171^a
	(0.0218)	(0.0173)	(0.0339)	(0.0328)	(0.0456)	(0.0461)
Observations	1,984	1,984	1,984	1,645	1,155	1,155
R^2	0.511	0.695	0.806	0.839	0.870	0.871
Property characteristics	✓	✓	✓	✓	✓	✓
Building characteristics	✓	✓	✓	✓	✓	✓
Location characteristics	✓	✓	✓	✓	✓	✓
Shopping street characteristics						✓
Year fixed effects	✓	✓	✓	✓	✓	✓
Shopping district fixed effects		✓				
Shopping street fixed effects			✓			
Intersection fixed effects				✓	✓	✓

Notes: Footfall is measured as the number of shoppers per day. Property, building, location and shopping street characteristics are mentioned in Table 5.9. In Column [4], we include observations within 100m of a shopping street interaction. In Columns [5] and [6], we reduce this distance to 50m. Robust standard errors clustered at the shopping street level are in parentheses. ^{a}, ^{b} and ^{c} indicates significant at 1, 5, and 10 percent level, respectively.

In Panel A, Table 5.17, we report the results using two interaction variables for the properties that belong to property owners owning a single property in the same shopping street, multiplied by the logarithm of footfall. The second interaction term uses properties that belong to property owners owning multiple properties in the same shopping street. In Column [1], we use our preferred specification which includes the full set of controls and 50m intersection fixed effects. Given the limited information on property owner names in our data and consequently, the low number of observations, we cannot consistently estimate the effect of log footfall on retail rents using this specification, which only includes observations within 50m of an intersection. For this reason, Column [2], includes observations within 100m from an intersection with the full set of control variables. In both Columns [1] and [2], the coefficients are virtually the same for single and multi-property ownership. The results of Panel A, Table 5.17, suggest that multi-property property owners value footfall in the same way as single property owners and thus, it might be expected that both behave in a similar manner.

Table 5.17: Regressions results for retail rents: ownership status

Dependent variable:	log(rent)	
	[1]	[2]
	OLS	OLS
	Panel A: Multi vs. single-property ownership	
Single-property owners×footfall (log)	0.139	0.217^a
	(0.0977)	(0.0741)
Multi-property owners×footfall (log)	0.136	0.225^a
	(0.102)	(0.0767)
Observations	558	760
R^2	0.942	0.922
	Panel B: Private vs. corporate ownership	
Private property owners×footfall (log)	0.229^a	
	(0.0404)	
Real estate companies×footfall (log)	0.229^a	
	(0.0398)	
Observations	1,458	
R^2	0.889	
Property characteristics	✓	✓
Building characteristics	✓	✓
Location characteristics	✓	✓
Year fixed effects	✓	✓
Shopping street characteristics	✓	✓
Intersection fixed effects	✓	✓

Notes: Multi-property (single) ownership is a dummy variable which takes the value one if a property belongs to a property owner who owns multiple (no other) properties in the shopping street that the property is located. Private property owners are those listed as private investors. Footfall is measured as the number of shoppers per day. Property, building, location and shopping street characteristics are mentioned in Table 5.9. In Column [1] we include observations within 50m of a shopping street interaction while in Column [2] we increase this distance to 100m. Robust standard errors clustered at the shopping street level are in parentheses. ^{a}, ^{b} and ^{c} indicates significant at 1, 5, and 10 percent level, respectively.

Panel B in Table 5.17 uses again two interaction terms of the properties that belong to private-property owners (versus real estate agencies, pension funds, construction companies etc.) who are (versus not) listed as private investors, multiplied by the logarithm of footfall. The coefficients of log footfall for private and commercial property owners are exactly the same. Overall, the results in Table 5.17 seem to confirm that any coordination among property owners in order to attract high-footfall generating activities and fully internalise the shopping externality is very unlikely to happen in the setting of the Dutch shopping streets.

5.8.8 Sensitivity analysis - number of shops and footfall

Here we will explore the extent to which footfall is superior to the use of the number of shops in the shopping street, as a proxy for shopping externalities. In Table 5.18 we add the logarithm of the number of shops that are located at the same street as the shop where a rent transaction took place, together with the logarithm of the average annual footfall[44]. We use the logarithm of the average annual footfall instead of log (annual) footfall to mitigate reverse causality and measurement error concerns as we discuss in detail in Section 5.4 and 5.8.4[45]. Following Nunn and Puga (2012), if the number of shops entirely accounted for the differential effect of log footfall between high and low footfall intersecting streets, the coefficient of log footfall should diminish and the log number of shops' coefficient should be statistically significant.

In Column [1], Panel A, we regress the log rent on the log average annual footfall controlling for property, building and location characteristics and time fixed effects. The coefficient of log average annual footfall is virtually unchanged compared to the coefficient we obtain using the same specification without including the log number of shops on the same street (0.365). Using shopping district fixed effects in Column [2] does not affect our results. In Column [3], where we include 100m intersection fixed effects, the coefficient of the log number of shops becomes marginally statistically significant. However, the marginal effect is relatively low while the log average annual footfall coefficient is very similar to the same specification without including the log number of shops (0.299). Finally, in Columns [4] and [5], we restrict our sample to observations within 50m from an intersection and in Column [5], we also add shopping street and other shop characteristics. Again, the results are very similar, suggesting that the number of shops cannot capture the full potential of shops to generate shopping externalities. These results suggest that the potential of shops to generate footfall is very heterogeneous. In other words, the elasticity of footfall with respect to shops, ε_{fnN}, may thus be very heterogeneous.

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[44] We matched each rent transaction (or each shop observation in the vacancy analysis) to all shops on the same street if they were non-vacant during the same or the previous year that the rent transaction took place (or for each shop observation).

[45] Using the log average annual footfall in our main specification yields relatively higher coefficients compared to when we use the log footfall. The results are included in Column [4], in Table 5.9 in Section 5.8.4.

Table 5.18: Regression results for retail rents: Footfall and number of shops

Dependent variable:	log(rent)				
	[1]	[2]	[3]	[4]	[5]
	OLS	OLS	OLS	OLS	OLS
Footfall (log) year average	0.374[a]	0.360[a]	0.282[a]	0.255[a]	0.252[a]
	(0.0227)	(0.0193)	(0.0312)	(0.0414)	(0.0417)
Number of shops in street (log)	-0.0285	-0.00255	0.0343[c]	0.0353	0.0370[c]
	(0.0221)	(0.0155)	(0.0195)	(0.0223)	(0.0225)
Property characteristics	✓	✓	✓	✓	✓
Building characteristics	✓	✓	✓	✓	✓
Location characteristics	✓	✓	✓	✓	✓
Shopping street characteristics					✓
Year fixed effects	✓	✓	✓	✓	✓
Shopping district fixed effects		✓			
Intersection fixed effects			✓	✓	✓
Observations	3,102	3,102	2,629	1,870	1,870
R^2	0.605	0.727	0.853	0.874	0.874

Notes: Footfall is measured as the number of shoppers per day. The number of shops in street (log) is the logarithm of the number of shops on the same street and in the same year that the rent transaction took place. Property, building, location and shopping street characteristics are mentioned in Table 5.9. In Column [3], we include observations within 100m of a shopping street interaction. In Columns [4] and [5], we reduce this distance to 50m. Robust standard errors clustered at the shopping street level are in parentheses. [a], [b] and [c] indicates significant at 1, 5, and 10 percent level, respectively.

Table 5.19: Regression results for vacant shops: Footfall and number of shops

Dependent variable:	dummy shop is vacant				
	[1]	[2]	[3]	[4]	[5]
	OLS	OLS	OLS	OLS	OLS
Footfall (log) year average	-0.0292[a]	-0.0294[a]	-0.0277[a]	-0.0278[a]	-0.0275[a]
	(0.00138)	(0.00118)	(0.00140)	(0.00165)	(0.00168)
Number of shops in street (log)	0.00927[a]	0.00399[a]	2.33e-06	-0.000372	-0.000597
	(0.00176)	(0.00102)	(0.00107)	(0.00123)	(0.00123)
Building characteristics	✓	✓	✓	✓	✓
Location characteristics	✓	✓	✓	✓	✓
Shopping street characteristics					✓
Year fixed effects	✓	✓	✓	✓	✓
Shopping district fixed effects		✓			
Intersection fixed effects			✓	✓	✓
Log-likelihood	-94,041	-92,184	-69,460	-44,013	-43,997
Observations	425,783	425,783	338,070	220,020	220,020

Notes: Reported coefficients are average marginal effects. Footfall is measured as the number of shoppers per day. The number of shops in street (log) is the logarithm of the number of shops on the same street and in the same year as each shop observation. Building, location and shopping street characteristics are mentioned in Table 5.10. In Column [3], we include observations within 100m of a shopping street interaction. In Columns [4] and [5], we reduce this distance to 50m. Robust standard errors clustered at the shopping street level are in parentheses. [a], [b] and [c] indicates significant at 1, 5, and 10 percent level, respectively.

In Table 5.19, we perform the same exercise as in Table 4 using a shop's incidence of being vacant, as the dependent variable. In the first two columns, it seems that the log number of shops has a positive and significant effect on vacancy rates. However, in the more conservative specifications with intersection fixed effects, the effect is very close to zero and not statistically significant. Hence, these results are in line with the results reported in Table 5.3 and our notion that footfall is the most appropriate measure to capture the heterogeneity of shopping externalities generated by each shop.

5.8.9 Sensitivity analysis - shops in distance thresholds

In Section 5.8.8, we tested whether footfall is superior to its main alternative, the number of shops on the same street that each shop is located. However, footfall exhibits substantial local variation within the same street. Therefore, one could argue that the aggregate number of shops at the street level cannot capture the local nature of shopping externalities. In Panel A, Table 5.20, we include both the log (average) footfall and the log number of shops for different distance thresholds. Following our baseline specification using the logarithm of the average annual footfall and the full set of control variables, year and 50m intersection fixed effects (shown in Column [1], Panel A, Table 5.20), we use four different distance thresholds for the shops that are located on the same street that the rent transaction took place. The distance thresholds range from 50m to 200m. Columns [2]-[4] present the results for each distance threshold. Regardless of the distance threshold used, the effect of the number shops is statistically significant. Nonetheless, the marginal effect of footfall is also highly statistically significant and virtually unchanged compared to the baseline specification in Column [1]. If the log number of shops accounted for the differential effect of log footfall between high and low footfall intersecting streets, the coefficient of log footfall should diminish. Thus, it appears that regardless of the area we use for the shop density measures, the latter cannot capture the heterogeneity of shops in generating shopping externalities.

In Panel B of Table 5.20, we only include the log number of shops in the same shopping street for the different distance thresholds, as a proxy for shopping externalities. Column [1] shows the baseline results when we include the log number of shops at the whole street where the rent transaction took place. Columns [2]-[4] are based on the same distance thresholds used in Panel A. In all columns, the elasticity of rents with respect to the number of shops is statistically significant and comparable to the results reported in Section 5.5.1.

We also repeat the same exercise for vacancies. Panel A, Table 5.21, shows the results when we include both the log average annual footfall and the log number of shops that are located on the same street as a shop that we observe (if it is vacant or not), for different distance thresholds. The coefficient of the log number of shops is statistically significant but very small between 50 and 150m, while for 200m or for the whole street, it is not even statistically significant. Moreover, the coefficient of log average footfall is essentially the same as in the baseline specification (Column [1]). Panel B, Table 5.21, reports the results when we only include the log number of shops in different distance thresholds. The results show that for each distance threshold chosen the elasticity of vacancies with respect to the number of shops is much smaller than the same elasticity with respect to footfall, suggesting that number of shops is an imperfect measure of shopping externalities.

Table 5.20: Regressions results for retail rents: Shops in distance thresholds

Dependent variable:	log(rent)				
	[1]	[2]	[3]	[4]	[5]
	OLS	OLS	OLS	OLS	OLS
	Panel A: Footfall and number of shops (log) (in same shopping street)				
Number (log) of shops within:	Baseline	50m	100m	150m	200m
Footfall (log) year average	0.272^{a}	0.260^{a}	0.246^{a}	0.245^{a}	0.243^{a}
	(0.0398)	(0.0394)	(0.0406)	(0.0415)	(0.0415)
Number of shops (log)		0.0618^{c}	0.0739^{b}	0.0642^{b}	0.0645^{b}
		(0.0343)	(0.0336)	(0.0306)	(0.0277)
Observations	1,870	1,869	1,870	1,870	1,870
R^2	0.874	0.874	0.874	0.874	0.875
	Panel B: Number of shops (log) (in same shopping street)				
Number of shops within:	Whole street	50m	100m	150m	200m
Number of shops (log)	0.100^{a}	0.126^{a}	0.167^{a}	0.153^{a}	0.144^{a}
	(0.0234)	(0.0385)	(0.0367)	(0.0319)	(0.0286)
Observations	1,870	1,869	1,870	1,870	1,870
R^2	0.864	0.862	0.864	0.865	0.865
Property characteristics	✓	✓	✓	✓	✓
Building characteristics	✓	✓	✓	✓	✓
Year fixed effects	✓	✓	✓	✓	✓
Location characteristics	✓	✓	✓	✓	✓
Shopping street characteristics	✓	✓	✓	✓	✓
Intersection fixed effects	✓	✓	✓	✓	✓

Notes: The number of shops includes all shops within each distance threshold that are located on the same street and in the same year that the rent transaction took place. Footfall is measured as the number of shoppers per day. Property, building, location and shopping street characteristics are mentioned in Table 5.9. In Columns [1]-[5], we include observations within 50m of a shopping street interaction. Robust standard errors clustered at the shopping street level are in parentheses. ^{a}, ^{b} and ^{c} indicates significant at 1, 5, and 10 percent level, respectively.

Table 5.21: Regression results for vacant shops: Shops in distance thresholds

Dependent variable:	dummy shop is vacant				
	[1]	[2]	[3]	[4]	[5]
	OLS	OLS	OLS	OLS	OLS
	Panel A: Footfall and number of shops (log) (in same shopping street)				
Number (log) of shops within:	Baseline	50m	100m	150m	200m
Footfall (log) year average	-0.0313^{a}	-0.0309^{a}	-0.0307^{a}	-0.0307^{a}	-0.0309^{a}
	(0.00201)	(0.00203)	(0.00207)	(0.00208)	(0.00208)
Number of shops (log)		-0.00441^{b}	-0.00398^{b}	-0.00265^{c}	-0.00169
		(0.00173)	(0.00170)	(0.00155)	(0.00148)
Log-likelihood	-44,042	-43,915	-44,006	-44,031	-44,033
Observations	220,049	219,503	219,974	220,020	220,020
	Panel B: Number of shops (log) (in same shopping street)				
Number of shops within:	Whole street	50m	100m	150m	200m
Number of shops (log)	-0.00527^{a}	-0.00624^{a}	-0.00926^{a}	-0.00842^{a}	-0.00753^{a}
	(0.00129)	(0.00186)	(0.00181)	(0.00164)	(0.00155)
Log-likelihood	-44,482	-44,373	-44,447	-44,469	-44,473
Observations	220,020	219,503	219,974	220,020	220,020
Building characteristics	✓	✓	✓	✓	✓
Year fixed effects	✓	✓	✓	✓	✓
Location characteristics	✓	✓	✓	✓	✓
Shopping street characteristics	✓	✓	✓	✓	✓
Intersection fixed effects	✓	✓	✓	✓	✓

Notes: The number of shops includes all shops within each distance threshold that are located on the same street and in the same year as each shop observation. Reported coefficients are average marginal effects. Footfall is measured as the number of shoppers per day. Building, location and shopping street characteristics are mentioned in Table 5.10. In Columns [1]-[5], we include observations within 50m of a shopping street interaction. Robust standard errors clustered at the shopping street level are in parentheses. ^{a}, ^{b} and ^{c} indicates significant at 1, 5, and 10 percent level, respectively.

5.8.10 Sensitivity analysis - effects on house prices

The dataset providing information on residential housing transactions is obtained from NVM, the Dutch Association of Real Estate Agents. The dataset provides information on about 90 percent of transactions between 2003 and 2014. We have information on the transaction price, exact location, and a wide range of house attributes such as size (in m^2), type of house, number of rooms and construction year. We merge the house price data to footfall data so that each transaction is within 25m of a shop in the Locatus data. One might expect that shopping districts like the ones we analyse have a purely commercial use. However, we have recovered information on building use for the same area that we analyse the effect of footfall on rents, which shows that about 50 percent of the building use is residential.

Table 5.22 reports the descriptive statistics. The average house price is about €200 thousand and the average price per m^2 is €2,333. As one may expect, residential properties are located in less busy shopping streets, with an average footfall of 9,256, i.e. about 30 percent less than in the Strabo dataset. The sample mainly includes apartments, as one expects for residential properties in shopping districts which are mainly located in the city centre. Similar to the Strabo dataset, about 25 percent of the properties are constructed before 1945.

Table 5.22: Descriptive statistics of NVM dataset

	mean	sd	min	max
House price (€)	201,156	95,503	40,000	950,000
Footfall	9,256	7,750	100	66,100
Size of property (in m^2)	91.32	34.89	26	250
Number of rooms	3.137	1.155	0	13
House type - apartment	0.901	0.299	0	1
House type - terraced	0.0672	0.25	0	1
House type - semi-detached	0.0256	0.158	0	1
House type - detached	0.00664	0.0812	0	1
Garage	0.102	0.303	0	1
Maintenance state - good	0.895	0.307	0	1
Central heating	0.863	0.344	0	1
Listed building	0.0299	0.17	0	1
Construction year 1945	0.259	0.438	0	1
Construction year 1945-1959	0.0729	0.26	0	1
Construction year 1960-1969	0.0553	0.229	0	1
Construction year 1970-1979	0.0922	0.289	0	1
Construction year 1980-1989	0.166	0.372	0	1
Construction year 1990-1999	0.164	0.371	0	1
Construction year 2000	0.19	0.392	0	1
Mall	0.0546	0.227	0	1
Corner shop	0.00664	0.0812	0	1
Sunny side of street	0.496	0.5	0	1
Pedestrian street	0.629	0.483	0	1
Shopping street length (in m)	385.3	270.2	34.68	1,269
Shopping street width (in m)	10.6	8.76	3	49.93
Distance to nearest intersection (in m)	83.02	116.3	4.241	2,961
Water within 50m	0.0687	0.253	0	1
Water 50-100m	0.089	0.285	0	1
In historic district	0.341	0.474	0	1
Distance to station (in m)	1,692	2,351	35.21	18,602

Notes: The number of observations is 9,947.

We now focus on the external effect of footfall through its effect on house prices. Retail concentration may have positive effects on residents through the positive effect of footfall on liveability. On the other hand, increased pedestrian traffic may generate congestion or noise, which could impose a negative external effect on residents, so the net effect is ambiguous. We employ the same identification strategy used for retail rents to estimate the external effect of footfall on residents that live in the same shopping streets as the ones used in our analysis of shop rents using residential house prices. Table 5.23 reports the results.

Table 5.23: Regression results for the housing market

Dependent variable:	log(house price)					
	[1]	[2]	[3]	[4]	[5]	[6]
	OLS	OLS	OLS	OLS	OLS	OLS
Footfall (log)	0.0237^a	0.0141^a	0.00326	-0.00160	-0.00120	-0.000895
	(0.00902)	(0.00478)	(0.00526)	(0.00576)	(0.00751)	(0.00748)
Property is in mall						0.0296
						(0.0223)
Property is on sunny side of street						0.0143
						(0.0103)
Property on the corner						0.0268
						(0.0344)
Shopping street width in m (log)						0.00494
						(0.00944)
Pedestrian street	-0.000438	-0.0101		-0.0213^c	-0.0104	-0.0123
	(0.0256)	(0.0105)		(0.0116)	(0.0150)	(0.0153)
Water within 50m	0.140^a	0.0618^a	0.0344^c	0.0405	0.0903^c	0.0926^c
	(0.0300)	(0.0147)	(0.0190)	(0.0286)	(0.0516)	(0.0511)
Water 50-100m	0.106^a	0.0336^a	0.00882	0.0170	0.0114	0.0133
	(0.0329)	(0.0111)	(0.0172)	(0.0199)	(0.0282)	(0.0278)
In historic district	0.0531^c	-0.0376^b	-0.00736	0.0118	0.0723	0.0685
	(0.0291)	(0.0188)	(0.0483)	(0.0300)	(0.0451)	(0.0448)
Distance to station (log)	0.0476^a	0.00930	0.00114	0.0719	0.154	0.147
	(0.0112)	(0.0157)	(0.0241)	(0.0541)	(0.111)	(0.110)
Housing characteristics	✓	✓	✓	✓	✓	✓
Building characteristics	✓	✓	✓	✓	✓	✓
Year fixed effects	✓	✓	✓	✓	✓	✓
Shopping district fixed effects		✓				
Shopping street fixed effects			✓			
Intersection fixed effects				✓	✓	✓
Observations	9,947	9,947	9,947	7,935	4,847	4,847
R^2	0.615	0.824	0.868	0.879	0.896	0.896

Notes: Footfall is measured as the number of shoppers per day. Housing characteristics include the size of property (in m^2), the number of rooms, the house type (apartment, terraced, semi-detached, detached), the maintenance state (if good) and the existence of a garage and central heating. Building characteristics include a dummy variable whether a building is listed and construction year dummies. In Column [4], we include observations within 100m of a shopping street interaction. In Columns [5] and [6], we reduce this distance to 50m. Robust standard errors clustered at the shopping street level are in parentheses. ^{a}, ^{b} and ^{c} indicates significant at 1, 5, and 10 percent level, respectively.

In Column [1], we include the logarithm of footfall and we control for housing, building, location characteristics and year fixed effects. The coefficient of footfall suggests that doubling footfall leads to an increase in house prices of 2.2 percent.

However, this coefficient is only marginally statistically significant. The positive effect may, however, be explained by the fact that areas with more footfall are generally located in or near the city centre. Such areas are often considered more attractive and therefore command higher housing prices. In Column [2], we therefore include shopping district fixed effects, implying that we identify the effect of footfall within shopping districts. The coefficient of footfall is then very close to zero and highly insignificant. The low magnitude of the standard errors provides convincing evidence for the absence of an external effect of footfall on residents. In other words, footfall is not a determinant of house prices or alternatively, the positive and negative effects of footfall perfectly counteract each other. Columns [3]-[6] confirm this finding. When we include street fixed effects in Column [3], or 100m intersection fixed effects in Column [4], the log footfall coefficient is essentially zero. The same holds in Column [5], where we use 50m intersection fixed effects, and in Column [6], where we additionally include street and shop characteristics in Column [6].

6 Concluding Remarks

People choose to live in cities because they offer them high production and consumption benefits. However, as the world becomes more and more urbanized, several urban costs — most notably traffic congestion and air pollution — have begun to undermine these benefits. European cities, in particular, increasingly face problems related to urban transport and if these external costs are not taken into account and internalised in the market, urban welfare will not be maximised. However, in order that transport-related externalities are given the necessary consideration, empirical evidence is needed that can quantify these externalities and the interactions between them. This PhD dissertation seeks to fill this void by estimating the effects of highway construction on suburbanization, on traffic congestion and on air pollution, as well as the interactions between congestion and pollution, on the one hand, and between congestion and accidents, on the other. Finally, in the last chapter, this dissertation quantifies shopping externalities, which are associated with a sustainable and very prominent form of transport in the city centres of Europe, namely walking.

Chapter 2 of this thesis estimates the joint effect of highway and railway construction on the suburbanization of the population of 579 cities located in 29 European countries between 1961 and 2011. The estimates suggest that an additional highway ray displaced about 9 percent of the central city population in Europe's cities, while, on average, we find no significant effect for railways. This effect is in line with estimates for the US and Spain (but note that it differs from those for China), suggesting that the underlying mechanism that 'drives' people to the suburbs in Europe is similar. However, the effect I report is not uniform across Europe and across different time periods. Note, for example, the effect was significantly higher during the period 1961-1981, when highway construction and urban growth in Europe were at their peak.

The heterogeneous analysis shows that highway construction in Eastern European cities led to more suburbanization, probably as a result of the fall of the Iron Curtain and the subsequent liberalization of the market in these countries. More interestingly, when our sample of cities is split into major cities during the Roman era, the Middle Ages, the Pre-Industrial and the Post-Industrial Revolution periods, we observe that in the cities of greater history, highways induced significantly less suburbanization. This finding indicates that historical (and other) amenities may have curbed suburbanization in Europe's historical cities. Finally, another result to emerge from this chapter is that the cities located in Objective 1 regions and which, therefore, received generous EU funding and invested primarily in highway construction, were not affected more in terms of suburbanization.

Suburbanization is a major externality, associated as it is with increases in greenhouse gas emissions, energy inefficiency and social segregation. Therefore, the findings of this chapter have important implications for the cities of Europe and may serve to assess, in part, EU policies related to transportation. First, the fact that expanding railway networks did not lead to suburbanization in Europe, despite the rail mode being a very popular and environmentally friendly means of transport, shows that EU measures to create a Single European Railway Area are a step in the right direction. Second, in relation to highway construction, the fact that cities located in Objective 1 regions did not suburbanize more also indicates that the highway investments made by the EU Regional and Cohesion Funds were not responsible for promoting the suburbanization of receptor cities. All in all, the results of this chapter provide a positive evaluation of EU transport infrastructure policy in terms of its effects on suburbanization. However, to assess these policies more exhaustively, we also need to take into account other externalities.

Chapter 3 investigates the effect of highway construction on highway congestion levels and the subsequent impact on urban air pollution. The first goal of the paper is to identify the causal effect of highway network development on levels of highway traffic. We overcome data availability issues by using GIS maps and we deal with potential endogeneity concerns by using four different historical transportation networks in Europe as instruments. The estimated elasticity of Vehicle Kilometres Travelled (VKT) with respect to highway lane km is in the range of 0.7-1. This estimate suggests that an increase in the supply of highways increases the level of traffic almost proportionally; thus, the level of traffic congestion remained roughly unchanged after the development of the highway network. After establishing the relationship between the supply of highways and traffic congestion, we estimate the subsequent effect of the increase in highway traffic on urban air pollution. Using a unique variable of emissions attributed to road transport and panel data techniques, I estimate the elasticity of nitrogen oxides, sulphur dioxide and fine particulate matter with respect to VKT, which is positive for all pollutants and highly statistically significant in most cases. I also estimate the same elasticity with respect to highway development, which suggests that a 10-percent increase in the highway lane km increases the emissions of all three pollutants by approximately 1 percent. Another interesting result from this heterogeneous analysis is that in cities with tolls or subways, traffic congestion and air pollution decreased as a result of highway development.

The EU is greatly concerned by the future of urban mobility and has set ambitious goals related to public transportation, fuel standards and road emissions for 2050. However, past EU policies, which included huge investments in highway infrastructure, have not yet been assessed with respect to their impact on traffic congestion and air pollution. Chapter 3 concludes that the expansion of the highway network did not effectively reduce traffic congestion while it contributed to urban air pollution. However, the increase in highway traffic was mainly driven by the capacity expansion and significantly less by the increase in the coverage of the highway network. This finding provides some support for the EU policies, which aimed primarily at increasing the connectivity between the countries and regions of Europe. Moreover, we derived some back-of-the-envelope calculations that suggest that the cost induced by investments in highways is relatively small. According to these calculations, the external cost imposed by highway development on the 545 cities in our sample as a result of the increase in nitrogen oxides, sulphur dioxides and fine particulate matter emissions is approximately €7.5 million, which is arguably quite small.

Therefore, chapter 3 suggests that the highway investments made by the EU have not substantially exacerbated air quality in Europe's cities. Likewise, it shows that in cities with tolls, the level of congestion actually fell after the expansion of the highway networks. This result is in line with the literature, which has long advocated for pricing as the best solution to congestion. In recent years, London, Oslo and Stockholm have introduced pricing schemes with very promising results in terms of both congestion and accidents. Finally, Europe is the world leader in subway systems. These rapid transit systems, together with the railways, offer a high-speed, congestion free and environmentally friendlier alternative to car travel, which does not require car ownership or a driving licence. In short, the findings of this Chapter are in line with the positive evaluation of EU policies reported in Chapter 2.

Chapter 4 estimates the relationship between highway accidents and traffic congestion, and vice versa, on England's highways between 2012 and 2014. I use publicly available 'big data' for highway traffic and accidents and merge these in a panel dataset that includes relatively small highway segments and traffic conditions in 15-minute intervals. Using dynamic panel techniques and the weekly and hourly stability of traffic patterns to isolate the effect of an accident on non-recurrent congestion, I find that an accident increases journey time by roughly 27 percent on average, when considering the duration of the effect. A further key finding is that the effect decays by 70-75 percent after the first quarter of an hour, which suggests that accident removal services are quite efficient in England. Furthermore, the effect of an accident on non-recurrent congestion is much more important in the recurrently congested parts of the network, whereas 'rubbernecking' (i.e. drivers heading in the opposite direction to the accident trying to view its aftermath) does not have any impact on highway congestion in England. The second part of this Chapter uses a very small sample of the full dataset (about 0.5 percent) to estimate the same effect and obtains very similar outcomes. This exercise suggests that refining the meaningful information is the real challenge of 'big data'. Finally, regarding the reverse effect of traffic congestion on the probability of an accident, I find evidence of a non-linear convex negative effect, i.e. more congestion is associated with a decrease in the probability of an accident. This result suggests that when the bidirectional interaction effects between the two externalities are accounted for, policies that aim to reduce the probability of an accident might have multiplicative benefits, while policies that focus on traffic congestion are not expected to reduce the probability of an accident substantially.

Finally, the ultimate goal of this Chapter is to offer some back-of-the-envelope calculations to support policies that seek to reduce the number of accidents. From such calculations, on average, an accident causes a 70-minute traffic delay per highway km for the users of that particular highway segment, while the delay rises to 160 minutes on recurrently congested segments. These results (assuming a value of time plus the cost of the accident) can be used to determine an upper cost limit for policies aimed at reducing the number of accidents. Finally, according to the findings of this chapter, traffic management authorities would benefit from focusing their accident prevention and accident removal efforts primarily on recurrently congested parts of the network.

Chapter 5 identifies shopping externalities in the full population of the main shopping streets of the Netherlands. Shopping externalities are the external benefits a shops receives from locating in a 'busy' shopping street. This externality arises from consumers' 'trip-chaining' behaviour in order to minimize their search and walking costs. We estimate the effect of footfall — the daily number of passing pedestrians — on shop rents and vacancy rates, which together determine the store owner's rental income. Using a novel identification strategy, we address endogeneity concerns by exploiting spatial differences in footfall between intersecting streets. Our estimates imply an elasticity of rental income with respect to footfall of 0.25, which is arguably high. The shop's marginal benefit of a pedestrian passing by is €0.004.

We also analyse the effect of high pedestrian movement on residential housing prices in the same shopping streets and find no effect of footfall. Thus, we conclude that our estimated externality is a net positive externality. Our results imply substantial subsidies to either incumbent shops or new shops on main shopping streets.

On average, a subsidy to new shops of 10 percent of the rent is welfare optimal, but the optimal subsidy to incumbent shops that generate above-average footfall levels is substantially higher. Such a subsidy could internalise the externality and increase urban welfare. Finally, such a policy could increase the variety of consumer goods available, increasing consumption amenities and liveability in the city centres of Europe.