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Shopping externalities and retail concentration: Evidence from dutch shopping streets[☆]

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Why do shops cluster in shopping streets? We argue that retail firms benefit from shopping externalities. We identify these externalities for the main Dutch shopping streets by estimating the effect of footfall – the number of pedestrians that pass by – and the number of shops in the vicinity on store owners' rental income. We address endogeneity issues by exploiting spatial variation within shopping streets combined with historic long-lagged instruments. Our estimates imply an elasticity of rental income with respect to footfall as well as number of shops in the vicinity of (at least) 0.25. We show that these shopping externalities are unlikely to be internalised. It follows that substantial subsidies to shop owners are welfare improving, seemingly justifying current policies. Finally, we find limited evidence for heterogeneity between retail firms located in shopping streets in their willingness to pay for shopping externalities.

1. Introduction

One of the main reasons that people choose to live in the 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 either in pedestrian shopping streets and shopping districts, often located in city centres, or in shopping malls in the suburbs. In Europe, shops are mostly concentrated in pedestrian streets. Walking in those streets is so important that the majority of all pedestrian movements occur while shopping.[3]

Arguably, the most important reason for retail firms to cluster is the presence of positive shopping externalities, which are generated by consumers' 'trip-chaining' behaviour. Shopping externalities have a simple logic. In retail markets where customers have to visit stores, transportation costs are 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 retail firm's productivity function depends on local footfall – the number of pedestrians that pass by a shop. Footfall tends to be higher in areas with more shops, since pedestrians are attracted to areas with many shops. Hence, the associated reductions in costs for consumers imply a positive shopping externality for retail firms, which is enhanced when multiple retail firms are located in close proximity (Eaton and Lipsey, 1982; Claycombe, 1991; Schulz and Stahl, 1996). Similar to other agglomeration advantages, these shopping externalities are expected to capitalise into store owners' rental income.[4]

In the empirical literature, however, little attention has been given to the importance of shopping externalities. The few studies that measure shopping externalities focus on U.S. shopping malls (see Pashigian and Gould, 1998). However, retail activity in European cities is mainly con-

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

4 In retail markets, agglomeration economies occur locally, so capitalisation into employees' wages is hardly relevant. In contrast, in non-retail markets, agglomeration advantages are more dispersed and mainly capitalise into wages (see *e.g.* Arzaghi and Henderson, 2008).

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centrated in shopping streets while shopping malls are far less common.[5] To the best of our knowledge, this is the first paper that quantifies shopping externalities in shopping streets.

Shopping streets are characterised by a different form of retail organisation than shopping malls. In contrast to shopping malls, we will show that property ownership in shopping streets is highly fragmented and that each shop is only a minor player locally. As a consequence, internalisation of shopping externalities is unlikely to occur in shopping streets.[6] Thus, public policies that foster retail concentration by providing subsidies are potentially welfare improving.[7]

We focus on the full population of main shopping streets in one country: the Netherlands. An important feature of shopping streets is that they are dominated by clothing stores (30%), cafés/restaurants (16%) and food stores (10%). The main strategy followed by the retail firms 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.* gas stations, movie theatres, or video retailers, see Netz and Taylor, 2002; Davis, 2006; Seim, 2006, respectively).

We contribute to the literature in three ways. First, we employ two measures of shopping externalities. Our first measure, *footfall*, is novel in the agglomeration literature.[8] We will argue that footfall captures shopping externalities rather precisely, also because measures of footfall predominantly include shoppers who visit several shops. As a second measure of shopping externalities we use the number of shops in the vicinity. The latter is a more standard proxy for externalities, which is in line with a large literature on agglomeration economies (see *e.g* Combes et al., 2008; Melo et al., 2009).

A second contribution of the paper lies in an explicit treatment of heterogeneity. One may suspect that there is heterogeneity in the benefits of shopping externalities. For example, shops that are part of a chain are more likely to locate in streets with high levels of footfall. Using semiparametric regression techniques we show that heterogeneity between firms in terms of *marginal* willingness to pay for footfall is limited. For example, shops belong to chains and other shops have roughly the same marginal willingness to pay for footfall. Note that this suggests that store owners cannot, or do not, discriminate between different types of retail firms in setting the rent.

The third, 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 we derive by estimating the causal effects of footfall on the rent paid by tenants as well as on the probability that a retail property lies empty. We will address several sources of endogeneity by exploiting spatial variation within shopping streets, combined with the use of a range of control variables (*e.g.*, size of property, shopping chain fixed effects) and an instrumental variable approach.

To be more specific, as emphasised in the agglomeration literature, both our measures of shopping externalities, *i.e.* footfall and number of shops in the vicinity, are essentially measures of spatial concentration that may be endogenous because they tend to be positively correlated to unobserved attractive location characteristics. We address this issue by focusing on shops that are located within the same shopping street and are therefore close to each other.[9] This strategy largely addresses the issue that footfall and number of shops in the vicinity are generated by local amenities and accessibility. We further control for a wide range of shop and street characteristics, as well as local amenities that may generate footfall that is not related to shopping, such as the number of schools, public buildings and religious buildings. Furthermore, using web-scraped data from *FlickR*, we control for the number of pictures taken by tourists and residents in the shopping street, which proxy for difficult-to-capture differences in amenities and attractiveness within a street (Gaigné et al., 2018).

Nevertheless, it is plausible that endogeneity issues are still present for both measures of shopping externalities due to measurement error and reverse causation issues, which would yield an *underestimate* of shopping externalities. We believe there are three reasons which may cause reverse causation. First, attractive, and therefore expensive, streets disproportionally attract shops with high-end brands (*e.g.* Louis Vitton, Giorgio Armani). These shops generate little footfall, so that there is a downward bias of the estimated footfall coefficient. In the Dutch context, the main example is the P.C. Hooftstraat in Amsterdam, but one observes the same phenomenon in well-known shopping streets in other countries (New Bond Street, London; Champs-Élysées, Paris; Via Monte Napoleone, Milan; Bahnhofstrasse, Zurich). Second, a retail firm that signs a contract to pay high fixed rents may be more likely to 'work hard' to generate footfall (and, therefore, sales) in order to ensure that it can afford these rents. Third, one may argue that there is a simultaneity problem (which usually does not occur in a hedonic price setting), because we consider the aggregate number of shops (and footfall) in a location, which would fall with rents, as in a standard textbook supply-demand setting with a homogeneous good.[10]

Endogeneity issues also play a role when focusing on the effects on vacancies. For example, a higher level of vacancies in a street will likely induce less foot traffic and, mechanically, reduce the small number of non-vacant stores in the vicinity.

To address these endogeneity issues, we will employ an instrumental variables approach using the exact location of cinemas in 1930. Because there is a high autocorrelation of retail locations, it appears that the number of cinemas in 1930 has a strong effect on footfall and on number of shops in the vicinity. This is not too surprising: for example, the maim shopping street in Amsterdam nowadays, de Kalverstraat, has been one of the main shopping streets for almost 300 years. Note that we identify these effects *within* shopping streets and control for the *current* location of cinemas, hence our instrument plausibly addresses the various endogeneity concerns.

We also make sure that our results do not depend on this particular choice of instruments by going back even further in time. We will use the number of shops in 1832 as an instrument. This instrument is particularly convincing because we show, using data on land values in 1832, that past attractive locations do not command higher retail rents nowadays. We are able to show that the location of shops in 1832 is a strong instrument (for footfall and number of shops), even when we control for the number of buildings in 1832. We discuss the assumptions underlying our identification strategy at length in the paper.

Our results show that footfall and number of shops have a strong positive effect on rental income with an elasticity ranging from 0.25–

[5] Shopping mall floor space per person is more than tenfold in the U.S. compared to Europe (2150 m^2 per 1000 people in the U.S. compared to 182 m^2 per 1000 people in Europe in 2011, see Cushman & Cushman, 2011).

[6] In shopping malls, store owners set the rent based on retail firms' revenues and thus, shopping externalities are internalised. More specifically, shopping mall real estate managers charge lower rents to footfall-generating shops (or 'anchor stores'), which could be regarded as a first-best subsidy. Hence, there is no room for public policy to improve welfare (Brueckner, 1993; Pashigian and Gould, 1998; Konishi and Sandfort, 2003).

[7] Many examples of such subsidies can be given, although these subsidies tend to be implicit. For example, in many European countries, in particular Germany, policies have created pedestrian areas. The latter implies implicit subsidies to local store owners, as the advantages are local, whereas the disadvantages of prohibiting car use in these areas fall on other agents. Subsidies to parking close to shopping clusters, and subsidies to park-and-ride facilities, including free public transport towards city centres, are other relevant examples.

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

[9] Shopping streets tend to be short; in our data, the median length is only 182 m.

[10] We would like to thank an anonymous referee for mentioning the latter two reasons.

0.50. When instrumenting, the estimates for both footfall and number of shops are somewhat higher. Thus, there are substantial external benefits from fostering footfall and retail concentration.

Based on these estimates, the optimal level of subsidy to store owners appears to be substantial, which potentially justifies a range of current policies that are especially beneficial to shopping streets. Current policies either subsidise shop owners, *e.g.* through pedestrianisation of popular shopping streets, or subsidise shoppers either through the provision of (subsidised, and sometimes even free) public transport to shopping districts or parking. Zoning policies which concentrates shopping into shopping streets (*e.g.* at the expense of residential housing), may also be beneficial.

Related literature. Our paper links and contributes to three strands of literature. First, it relates to the literature on spatial competition and 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. 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 on incumbent (discount) supermarkets and small grocery stores. Clapp et al. (2016) focus on openings and closings of multi-line department stores and find evidence for strong negative competitive effects within the own branch. Zhou (2014) shows that multi-product 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. However, these papers ignore that many shops benefit from each other when located close to each other.

Our paper also relates to a second literature that explicitly focuses on the benefits of agglomeration for firms. There is ample evidence that firms that locate close together benefit through input- and output sharing, labour market pooling and knowledge spillovers (Marshall, 1890). Current evidence suggests that the elasticity of productivity with respect to density is around 0.05 (see *e.g.* the meta-analysis by Melo et al., 2009). This literature typically focuses on the manufacturing industry. Compared to that literature, we find substantial agglomeration elasticities, which are (at least) 5 times larger. This is in line with Koster et al. (2014), who show that agglomeration economies are much more important for retail firms. Teulings et al. (2017) use a monocentric model where customers arrive in a central location and have to walk to shops around this location. Locations that are further away from the centre are therefore expected to be less profitable. Their empirical evidence indicates that rents in shopping districts are indeed higher close to the centre.[11]

Our findings also contribute to a more policy-oriented literature studying the effectiveness of retail policies, in particular towards the effects of the opening of large 'big-box' retailers near the urban fringe (Sanchez-Vidal, 2016). Some studies demonstrate 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). By contrast, our study shows that current policies that implicitly subsidise store owners in shopping streets are potentially welfare improving, as they make it more attractive to open additional stores in dense areas. This conclusion is consistent with the shopping mall literature, which shows that the provision of substantial rent discounts to 'anchor' stores in US shopping malls to internalise externalities is common practice (Pashigian and Gould, 1998; Gould et al., 2005). Shopping externalities may justify public policies that pedestrianise shopping streets in order to effectively cluster shops and internalise external benefits.

[11] To capture this phenomenon, we control for walking time to the centre of a shopping district (*i.e.* the location with the highest footfall).

The remainder of this paper is structured as follows. In Section 2 we discuss the theoretical framework that guides the empirical results. Section 3 introduces the data and reports descriptive statistics, followed by a discussion of the econometric framework in Section 4. In Section 5, we present and discuss our results, including the estimates of the optimal subsidy. Section 6 discusses an extension where we allow for a heterogeneous version of our model. Also the main sensitivity analyses are discussed. The latter are described in more detail in Appendix D. We draw conclusions in Section 7.

2. Theoretical framework

2.1. Rental income, rents and vacancies

We aim to measure the presence of shopping externalities by estimating the effect of footfall or number of shops on (expected) rental income of shop i, denoted by I_i.[12] Footfall is defined as the number of pedestrians that pass a shop (per unit of time). We assume that pedestrians walk through one shopping street and pass all shops in this street.[13] Let $s = \{f, N\}$ denote shopping externalities, where f refers to footfall and N to the number of shops in the shopping street.[14] In what follows, we make a distinction between store owners that own shops and retail firms that rent shops. Shops can be either occupied by retail firms or vacant. Store owners of vacant shops need advertising services to find a new tenant, which is costly. Given rent p_i and vacancy rate v, rental income of a shop is given by:

$$I_i = p_i(1 - v) - c_i v, \tag{1}$$

where $p_i(1 - v)$ is rental income when the property is let to a retail firm and $c_i v$ is the advertising costs. It seems reasonable to assume that, at least in the long run, the advertising costs c_i are proportional to p_i, so $c_i = \kappa p_i$, where $\kappa > 0$. Because vacancy rates tend to be small (usually smaller than 10% in our data), $\log(1 - (1 + \kappa)v) \approx -(1 + \kappa)v$. Hence, the log of rental income is then (approximately) equal to $\log p_i - (1 + \kappa)v$.

If shopping externalities have an effect on the rent and vacancy rate, it follows that the effect of log shopping externalities on log rental income can be written as the sum of the marginal effect of log shopping externalities on the *logarithm* of rent and the marginal effect of log shopping externalities on the *level* of the vacancy rate:

$$\frac{\partial \log I_i}{\partial \log s} \approx \frac{\partial \log p_i}{\partial \log s} - (1 + \kappa)\frac{\partial v}{\partial \log s}. \tag{2}$$

Note that a standard hedonic model cannot be used to predict $\partial \log I_i / \partial \log s$ because vacancies are not incorporated in such a framework. In Appendix A.1 we therefore set up a search and bargaining framework, where store owners have to decide on the level of advertising which is necessary to find retail firms searching for retail space. Consequently, in this set up, vacancy rates are endogenously determined. Furthermore, it is assumed that owners with vacant properties and retail firms searching for retail space bargain about the rent level when they make contact with each other, while both are uncertain how much time it takes to make another contact. We then show that, in equilibrium,

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

[13] This assumption is made for convenience only, as it is plausible that shoppers usually walk through several shopping streets.

[14] In our empirical application, we take into account that shoppers usually walk through several shopping streets. We will measure N based on the number of shops within a given radius of a shop. The area defined by this radius may include several shopping streets.

$\partial \log p_i/\partial \log s > 0$ and $\partial v/\partial \log s < 0$.[15] Hence, in equilibrium, shopping externalities not only increase rents, but also increase vacancy rates. If we assume that $\kappa = 0$, we get the lower bound estimate of the effect of footfall on log rental income. We will make that assumption but also calculate the effect of footfall on rental income for different values of κ (based on the advertising costs for Dutch retail properties).

2.2. Welfare and retail policies

Let us now focus on welfare effects of retail policies. Intuition suggests that retail policy can be welfare improving, because retail firms generate footfall, and policies may influence the spatial concentration of retail films. To investigate this, we will assume here that the effect of shopping externalities is entirely driven by the effect of footfall on rental income. The effect of number of shops is only indirect, as it affects footfall, but not rental income directly. Hence, more formally, we assume that for a shop i the rental income is:

$$I_i(f, N) = I_i(f(N)) = p_i(f(N))(1 - v(f(N))) - c_i v(f(N)). \tag{3}$$

Store owners may decide to increase the supply of stores in a street (extensive margin) or/and reduce existing vacancies (intensive margin).[16] Hence, the number of shops, as well as the number of vacant shops in a shopping street, is endogenous. Both decisions imply an externality because footfall depends positively on the number of shops and negatively on the vacancy rate. Here we investigate the welfare effect of increasing the number of *occupied* shops.[17] Combined with the assumption that we focus on a competitive market, this implies that it is not necessary to distinguish between shop owners and retail firms.

The number of occupied shops, N, in a shopping street is endogenously determined. For now let us assume that shops are homogeneous. The per-period marginal construction and maintenance costs of N shops are equal to $C(N)$, which is increasing in N. Footfall in a street, f, is an increasing function of the number of shops in the shopping street, so $\partial f/\partial N > 0$, as well as the footfall generated *per individual shop*, denoted by $\tilde{f}_i$, so $\partial f/\partial \tilde{f}_i > 0$. The shop's costs of generating footfall are equal to $q_i(\tilde{f}_i)$, which is an increasing and convex function of $\tilde{f}_i$. For example, shops may generate footfall by advertising, which is costly. Welfare in the shopping street is now defined by:

$$\int_0^N I_i(f(N, \tilde{f}_i))\mathrm{d}i - C(N) - \int_0^N q_i(\tilde{f}_i)\mathrm{d}i. \tag{4}$$

The welfare-optimal number of shops is then determined by the following equation:

$$I_i + N\frac{\partial I_i}{\partial N} = C'(N) + q_i(\tilde{f}_i), \tag{5}$$

where $i = N$. The above equation states that the marginal expected income of N is equal to the sum of the marginal cost of opening an additional store and the cost of generating footfall. Note that the marginal benefit of opening a shop – *i.e.* the left-hand side of the above equation – is an increasing function of N. This is intuitive, because each shop that opens up in the shopping street benefits from the footfall generated by nearby retail firms. This implies that the cost of opening a shop ($C(N)$) must be strongly convex, so that the second-order condition holds.[18]

To calculate the optimal subsidy to store owners is straightforward. The marginal store owner will ignore the term $N(\partial I_i/\partial N)$ when considering to increase (or decrease) the supply of shops. Hence, the marginal external benefit of opening up a new shop is equal to:

$$N\frac{\partial I_i}{\partial N} = I_i \cdot \varepsilon_{I,N}(i) > 0, \tag{6}$$

where $\varepsilon_{I,N}(i)$ denotes the elasticity of rental income of shop i with respect to the number of shops. In our empirical application we will estimate the elasticity $\varepsilon_{I,N}(i)$.

Importantly, we will assume that this shopping externality is not internalised by store owners. This seems a reasonable assumption for shopping streets due to the fragmented ownership of shops, which is, as we will show, supported by our data.[19] Given this assumption, the Pigouvian subsidy to the marginal store owner in the first-best optimum is $\varepsilon_{I,N}(i)$ times the rental income of a shop. In our empirical analysis, we are able to calculate this subsidy as we will observe rental income and estimate $\varepsilon_{I,N}(i)$.

We will now determine the welfare-optimal level of footfall in a street, which is the result of the self-chosen footfall level by an individual firm. The welfare-optimal level at the street level, f, can then be derived by maximising welfare with respect to the individual level of footfall, $\tilde{f}_i$. It can be derived by an equation, which states that the marginal expected income of individual footfall, $\tilde{f}_i$, is equal to its marginal cost:

$$\frac{\partial I_i}{\partial f}\frac{\partial f}{\partial \tilde{f}_i} = q_i'(\tilde{f}_i). \tag{7}$$

We will now assume that shops are homogeneous. This implies that $\tilde{f}_i = \tilde{f}$, $I_i = I$, $q_i(\tilde{f}_i) = q(\tilde{f})$ and $\varepsilon_{I,N}(i) = \varepsilon_{I,N}$. It follows that $f = N\tilde{f}$ and Eq. (7) can be written as:

$$\frac{\partial I}{\partial f}N = q'(\tilde{f}). \tag{8}$$

The left-hand side of this equation depicts the marginal benefit of footfall to society generated by an individual shop. The marginal benefit of footfall to a shop is equal to $\partial I/\partial f$ (as the shop ignores that other shops in the same street benefit from the footfall). Given that the number of shops is usually large in a street (and far above one), the external benefit of footfall, and hence the welfare-optimal subsidy, is equal to:[20]

$$\frac{\partial I}{\partial f}N = \frac{N \cdot I}{f}\varepsilon_{I,f} > 0, \tag{9}$$

[15] Another issue is that footfall depends on the number of vacancies, as vacant shops will attract fewer customers. We address this issue in Appendix A.2 where we endogenise footfall so that it is dependent on vacancy rates. We show that the main consequence of ignoring this endogenous relationship for our empirical investigation is that one underestimates the effect of footfall on rental income. We come back to this issue in Section 4.

[16] The supply of shops can be increased by building new properties for retail activity or by converting existing space to retail use. Existing vacancies may be reduced *e.g.* by increasing the search effort.

[17] The welfare effect of filling an existing vacancy and increasing the supply of shops is equivalent given two conditions. The first is that filling an vacancy and increasing the supply of shops has an identical effect on footfall, which seems reasonable. The second condition is that, in the absence of a footfall externality, the decision to fill a vacancy is socially optimal, implying that there are no other externalities. Note that store owners with vacancies may increase their advertising expenditure, which increases the probability to find a tenant. That, in turn, may increase footfall for other shops. It is in general not clear whether store owners with vacancies choose the socially optimal advertising expenditure because of negative congestion and positive search externalities. See, for example, Hosios (1990) for an analysis of optimal advertising expenditure in the labour market. We leave this issue for further research.

[18] The latter is likely true because in shopping streets, shops almost always occupy the ground floor of a building (see Liu et al., 2016), which implies that the number of shops is restricted by the length of the street.

[19] In contrast, 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 a shopping mall's welfare.

[20] To evaluate the exact general-equilibrium welfare improvements associated with such a subsidy is out of the scope of the current paper and depends on, among others, to what extent such a policy leads to openings of new retail firms in the economy, and to what extent existing shops move to other shopping streets due to the subsidy. If the demand for street shopping is inelastic, then the welfare gains will mainly come from travel cost savings. If it is elastic, then the welfare gains will be higher. Eq. (6) does not make clear whether shops in shopping streets with many shops nearby should receive higher levels of subsidies than shops in streets with few shops. Hence let us analyse the difference in optimal subsidy level between two shopping streets, labelled 1 and 2, which can be

where $\varepsilon_{I,f}$ denotes the elasticity of rental income with respect to footfall. In our empirical application we will also estimate the elasticity $\varepsilon_{I,f}$. We will show that in our data $\varepsilon_{I,f} = \varepsilon_{I,N}$ (which is in line with our assumption that footfall is proportional to the number of shops in the street).

We previously assumed that retail firms are homogeneous. It is clear however that retail firms may differ in the extent that they benefit from the presence of other shops, *i.e.* footfall. Hence, we will allow $\varepsilon_{I,N}$ and $\varepsilon_{I,f}$ to be retail firm-specific. We will see however that heterogeneity of retail firms with respect to the marginal willingness to pay for footfall generated by other shops is quite limited in shopping streets, which suggests that modern shopping streets attract relatively similar shops. In Section 6 we will investigate this further.

3. Data and descriptives

3.1. Data

We base our empirical analysis on various 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 2003 to 2015. The dataset contains information about annual rents (reported at the moment that the contract has been signed) and rental property attributes, such as address, size (gross floor area in m^2) and whether the building is newly constructed or renovated. In our data, all rents are independent of retail revenues in line with common practice for the Netherlands.[21] From the *Strabo* dataset, we exclude observations for which no rent is reported (27.8% of all shops in the dataset).[22] The rental transactions are then matched to data from the *Building Registry* (BAG), which provides the exact location and construction year for all buildings in the Netherlands. Using a 25 m distance threshold, we matched almost all of the *Strabo* 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 (Carlino and Saiz, 2008). The dataset is also merged with detailed land use data from *Statistics Netherlands*. The latter data enable the calculation of distance to the nearest railway station. To determine amenities and accessibility of the shop, we first gather data from *Duo* on the locations of kindergartens, primary and secondary schools in the street. From *Imergis* we use the location of public buildings (town halls, police and fire stations) and religious buildings. Using *OpenStreetMap* we also determine the number of bus stops in a shopping street. It is hard to control for all relevant amenities. We therefore gather data from Eric Fisher's *Geotagger's World Atlas*, which contain all the geocoded pictures on the website *Flickr*. These pictures should be a reasonable proxy for a location's attractiveness (see Gaigné et al., 2018).

The other main dataset is a retail dataset obtained from *Locatus*, which contains the entire population of shops. For each establishment, we know whether a shop is vacant or occupied and its retail sector (when occupied), and whether a retail firm occupying a shop is part of a chain. Furthermore, we know the 8-digit retail sector, which provides very detailed information. For example, we do not only know whether a retail firm sells apparel, but also whether it targeted at kids, women or men.

The *Locatus* dataset also provides 3936 annual counts of footfall in all main shopping streets of the Netherlands from 2003 to 2015, so that we have more than 50 thousand footfall counts over the whole study period. The measurement points are not the shops themselves, but selected points in pedestrian streets or at sidewalks with an average distance of 45 m between them. The main shopping streets contain about 13.4% of all shops in the Netherlands. The annual footfall data, provided by *Locatus*, is based on footfall measurements collected on four 'regular' Saturdays (two in Spring and two in Autumn) at four different hours of the day. Using these 16 measurements, *Locatus* calculates the average footfall 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 45 m.

For most observations (about 90%) in the *Strabo* dataset we have detailed information on the name of the retail firm renting the store. We have matched each rent transaction in the *Strabo* dataset to a shop in the *Locatus* dataset in order to recover the 8-digit sector and whether the firm is part of a chain.

We have defined a shopping street as a continuous straight street (or slightly curved street) based on manually created GIS polylines for all streets for which there is at least one location where footfall data is available. Using this definition, based on the above-discussed *Building Registry* dataset, we define 1253 unique shopping streets. The median length of these streets is 182 m, so streets are short. The median width of a street is 12 m. This underlines that we focus on shopping streets within European cities with short and narrow streets, rather than, for example, U.S. cities, which usually have longer and wider streets.

We illustrate the data in Fig. 1 based on a sample of our data for the city centre of Amsterdam, which contains two shopping districts including the city centre. This area is the busiest shopping district of the Netherlands with the highest level of daily footfall. It shows that there is substantial spatial variation in the annual average of footfall both *within* shopping streets and *between* shopping districts. Moreover, rent transactions (the stars in the map) are numerous and cover almost the whole shopping district. The centre of shopping districts is determined by taking the location with the highest footfall within the district.

For our identification strategy, explained later on, we will also rely on historic data going back to 1930 or even 1832. We gathered information on the exact location of cinemas in 1930, as well as in 2010 (in 1930 there were 315 cinemas, while in 2010 this has been reduced to 180) from *SpinLab*. For maps and descriptive statistics, we refer the reader to Appendix B.1.

We further use data from *HISGIS*, which provides geocoded data of the first Dutch census of 1830. It is the oldest nationwide registration system of property and land ownership. We have information on the footprint size of each building. Moreover, for each parcel we know the owner and the owner's occupation. Based on the occupation and whether there was a building located on the parcel, we determine whether a building was used as a shop. We explain this in more detail in Appendix B.1. For each parcel we also have information on the so-called *Cadastral Income*, which is a proxy for the land value, as land taxes were based on this value. Again, we report descriptives and maps in Appendix B.1.

3.2. Descriptives

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 1 summarises the descriptive statistics for the *Strabo* dataset. We have 4738 rental transactions located in 131 shopping districts. The mean rental price is equal to € 51,449. Footfall exhibits substantial variation ranging from 100 to 71,000 pedestrians passing by each day. Mean daily footfall is 13,328 with a standard deviation of 8935.

In the theoretical analysis, we focused on the number of shops within a shopping street. Nonetheless, we want to take into account shoppers

written as $I^1 \cdot \varepsilon^1_{I,N} - I^2 \cdot \varepsilon^2_{I,N}$. Now suppose that street 1 contains more shops than street 2. This equation shows that larger shopping streets must receive larger subsidies as long as the elasticity of rental income with respect to number of shops is positive (implying $I^1 > I^0$) and non-decreasing in the number of shops (so, $\varepsilon^1_{I,N} \geq \varepsilon^2_{I,N}$). We will see that both conditions are fulfilled. This implies that subsidies to store owners in large shopping streets must exceed those in small shopping streets.

[21] Moreover, our identification strategy will address the issue of 'percent rents'.

[22] We do not find evidence for differences between the transactions with and without information on rents, although the latter seem to refer to somewhat larger rental properties.

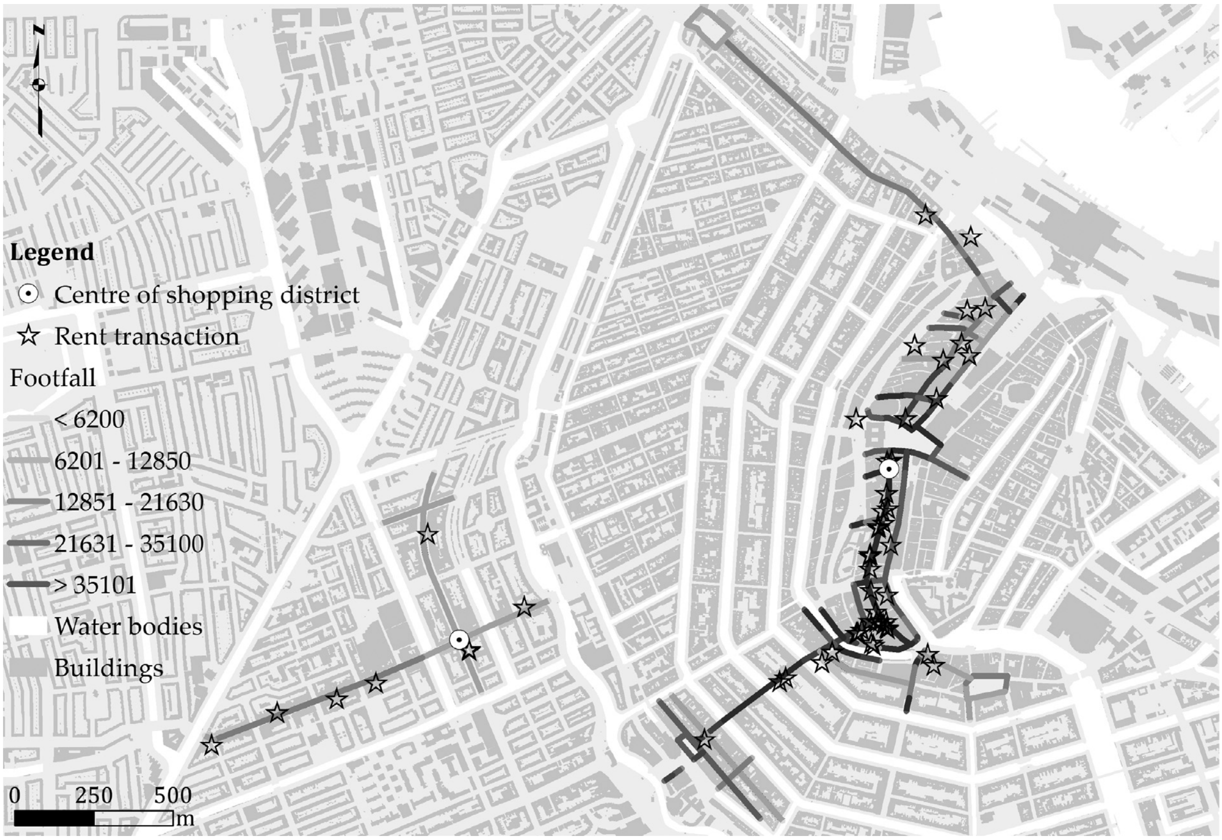


Fig. 1. Sample map for the Amsterdam city centre.

Table 1
Descriptive statistics for the Strabo dataset.

	(1) mean	(2) sd	(3) min	(4) max
Rent *(in €)*	51,234	83,227	3400	2,700,000
Footfall *(number of shoppers per day)*	13,328	8935	100	71,000
Number of shops, <200 m	132.2	68.89	0	406
In pedestrianised street	0.862	0.345	0	1
Size of property *(in m^2)*	201.2	268.2	19	7200
Building – new	0.0112	0.105	0	1
Building – renovated	0.00708	0.0839	0	1
Construction year	1933	85.30	1325	2016
In historic district	0.474	0.499	0	1
Number of photos in street, <200 m	283.4	451.3	0	5181
Religious buildings in street, <200 m	0.714	1.058	0	6
Bus stops in street, <200 m	0.861	1.478	0	10
Public buildings in street, <200 m	0.0955	0.302	0	2
Schools, kindergartens in street, <200 m	0.153	0.515	0	4
Railway stations, <200 m	0.0247	0.155	0	1

Notes: The number of observations is 4378.

who walk through several streets. Therefore, we will measure the number of shops within a radius of 200 m from each shop location (but we will also experiment with other thresholds). The area defined by a 200 m radius may contain several shopping streets. There are on average 132 shops within 200 m, hence the number of shops within the vicinity is very high. Moreover, the large majority of shops are small, with a mean of 201 m^2. One implication of this is that individual shops usually contribute little to overall footfall, which makes it very unlikely that shop owners within shopping streets have market power and suggests that the retail real estate market is highly competitive.

The correlation between footfall and number of shops in the vicinity is moderate ($\rho = 0.375$). About 1% of shops are either new or renovated when the rental transaction took place. About 6% of the shops is built before 1832, while about half of the shops are built before the Second World War. Therefore, almost half of the observations is in historic districts. In Table 1 we also provide information about pedestrian streets. We do not control for pedestrian streets in the analysis (pedestrianisation might be endogenous), but note that the large majority of shops, about 85%, are located in streets that are pedestrianised.

In our data, we will distinguish between 153 shopping districts (a shopping district contains about 260 shops, on average). A substantial proportion of shopping districts (about 45%) are not within 5 km of the centre of a city. Hence, in terms of shopping districts, we have a good representation of shopping districts that are out of the city centre. However, the proportion of shops not within 5 km of the centre of a city is much smaller and only about 25% (as suburban shopping districts tend to be smaller).

In Table 2, we report descriptive statistics for shops in the *Locatus* dataset. We have 410,544 observations of shops in 133 shopping districts and 1243 shopping streets. About 6% of the shops are vacant.[23] The descriptive statistics of the location variables are comparable to the descriptive statistics for the *Locatus* data.

We focus on shops in shopping streets that presumably aim to benefit from footfall, hence our sample of shops is far from a random sample of shops. This is particularly the case for clothing. Clothing stores are the most common branch (30% of shops) – almost 4 times higher than the national average. However, this is not the case for restaurants and

[23] The *Strabo* dataset contains a considerably higher share of shops in older buildings (particularly constructed before 1940) than the *Locatus* dataset, which contains the full population (the main explanation for this difference is 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 excluded and shops with long rental contracts are under-represented).

Table 2
Descriptive statistics for the Locatus dataset.

	(1) mean	(2) sd	(3) min	(4) max
Shop is vacant	0.0610	0.239	0	1
Footfall *(number of shoppers per day)*	12,379	10,732	100	102,600
Number of shops, <200 m	129.0	62.77	0	439
In pedestrianised street	0.843	0.364	0	1
Building size *(in m^2)*	179.9	1089	25	27,694
Construction year	1967	35.83	1445	2012
In historic district	0.403	0.490	0	1
Number of photos in street, <200 m	280.5	560.7	0	5493
Religious buildings in street, <200 m	0.755	1.096	0	7
Bus stops in street, <200 m	0.705	1.338	0	10
Public buildings in street, <200 m	0.0951	0.301	0	2
Schools, kindergartens in street, <200 m	0.185	0.552	0	4
Railway stations, <200 m	0.0193	0.138	0	1

Notes: The number of observations is 410,544.

cafés: the share of restaurants and cafés (16%) is exactly equal to the national average. In both branches the degree of product differentiation is extremely high.

In Europe, shopping districts usually exhibit a pattern of mixed land uses. This is also the case in the Netherlands. Using information from the *Building Registry* on buildings within 25 m of a shopping street, it appears that a small minority, only about one quarter, of the properties is used for shopping, whereas almost half of the properties is residential, and the other quarter is used for other purposes (*e.g.* offices, public services).

The observation that shops are typically small suggests that the retail real estate market is highly competitive, but this is only true when store ownership (and therefore land ownership) within shopping streets is highly fragmented. This appears to be the case. This claim is based on the *Strabo* dataset for which store owner type and regularly even owner's name is reported (we know the store owner type for about two thirds of the observations and store owner's name for about one third of the observations). Using information about names, it appears that on average only 28% of the shops within a shopping street belong to a store owner who owns more than one shop in the same shopping street.[24] On average, store owners possess 1.32 shops per street, which is a low number given that there are on average 55 shops per street. This indicates that it is highly unlikely that the shopping externality that we measure is internalised.

We also use information about owner type that distinguishes between private-store owners, real estate agencies, pension funds, construction companies etc. Only 34% of shops are owned by real estate investors. Thus, the large majority of shops are owned by individual private investors, which further supports our claim that ownership of shops within shopping streets is highly fragmented. We also gathered information from Menger (2014) on localised shop associations, in which externalities may be internalised (but which may not engage in anti-competitive activities). These shop associations are area-based and organise *e.g.* collective marketing and provide public goods such as Christmas lightning. On average 14% of the retail firms are part of such associations.

4. Econometric framework

4.1. Rents

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 in shopping street j in year t. We use two proxies for shopping externalities $s_{ijt} = \{f_{ijt}, N_{ijt}\}$: footfall and the number of shops in the vicinity. Furthermore, let x_{ijt} be other shop and location characteristics (*e.g.* shop size, construction year, historic district etc.). The basic equation to be estimated yields:

$$\log p_{ijt} = \alpha \log s_{ijt} + \beta x_{ijt} + \theta_t + \epsilon_{ijt}, \tag{10}$$

where α and β are parameters to be estimated, θ_t are year fixed effects and ϵ_{ijt} is a random error term.

There are a number of concerns when using footfall as a proxy for shopping externalities. One may be worried that non-shoppers and one-stop shoppers may be included in footfall. Arguably, this is a minor issue because our measure of footfall predominantly includes shoppers who visit several shops.[25]

Another concern is measurement error. Measurement error is likely present for a range of reasons. First, for footfall, we only have measures on specific Saturdays, which may not be representative for the average Saturday, or for other days of the week. We believe that the extent of measurement error here is not too serious. Second, the number of shops in the vicinity may have substantial measurement error, because shops differ substantially in size. In specifications with street fixed effects, the bias caused by measurement error within the same street may become more pronounced. Most likely, we will get underestimates because of measurement error. To take measurement error into account, we will use an IV approach, as explained later on.

A more serious concern is potential simultaneity. Rents of shops may determine the type of shop, whereas shops vary in the extent of footfall they generate; hence there is potentially an effect of rents on footfall. Furthermore, the level of rent may determine the size of shops and therefore, the number of shops in vicinity. Hence, the number of shops may also be endogenous. Moreover, there may be unobserved store and location characteristics that are correlated with footfall and the number of shops in the vicinity. For example, a store's 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 shops) and customers value building quality, a naïve hedonic regression would suffer from omitted variable bias.

To make the identification of a causal effect of shopping externalities more plausible, we take a number of steps. First, we include shopping district fixed effects, implying that we identify the differences in footfall within the shopping district. This approach mitigates the problem of unobserved endowments, but may not solve the problem entirely because shopping districts may be quite large. We therefore also propose another identification strategy using spatial variation in local footfall within shopping streets. We will also control for web-scraped data on pictures taken by residents and tourists and count the number of pictures in each shopping street. The idea is that locations with an abundant supply of aesthetic amenities may have a high picture density and a high number of non-shoppers or one-stop shoppers.[26]

Second, to mitigate the issue that specific retail firms that generate a lot of footfall may negotiate lower rents, we use information on the 8-digit retail sector and whether the retail firm is part of a chain. $\psi_{i\in k}$ is then a fixed effect for each retail sector by chain combination, denoted by k. We then estimate:

$$\log p_{ijt} = \alpha \log s_{ijt} + \beta x_{ijt} + \varphi_j + \psi_{i\in k} + \theta_t + \epsilon_{ijt}, \tag{11}$$

[24] Given that this dataset only contains rental transactions, it is likely that the share of multi-store owners is overrepresented in our sample, because this share is likely lower for owned shops.

[25] Since the probability that a shopper is included in our measure of footfall is (approximately) proportional to the number of shops visited, the share of non-shoppers or one-stop shoppers should be much smaller than the share of multi-stop shoppers. For example, if 25% of footfall were one-stop shoppers, and the other 75% visit four shops, then the proportion of one-stop shoppers would be only 7.8%.

[26] Ahlfeldt (2016) and Gaigné et al. (2018) show that there is a strong positive correlation between picture density and amenities, such as historic buildings, restaurants and parks. The number of pictures in a street is likely a good proxy for foot traffic that is not necessarily related to shopping.

where φ_j are shopping street fixed effects.

We improve on this specification by estimating specifications where *(i)* we include retail firm fixed effects to further control for unobserved retail firm characteristics, *(ii)* only focus on small shops, where it is unlikely that the retail firm will contribute substantially to footfall, and *(iii)* focus on firms that are not part of a larger retail chain, which are more likely to advertise and therefore to generate footfall. We also show that *(iv)* when flexibly controlling for the walking time to the city centre, the results are essentially unaffected.

Still, one may not be fully convinced that Eq. (11) fully addresses the issue of unobserved store and locational characteristics. Furthermore, it is plausible that reverse causation still plays a role.

In order to address such concerns, we will also pursue an instrumental variables strategy. We will use locally long-lagged instruments, which do not affect directly today's rents, but determine contemporary concentration of shops and, therefore, footfall. More specifically, conditional on shopping street fixed effects, we will first use the number of cinemas in 1930 in the vicinity as an instrument for the current number of shops in the vicinity and footfall.

We use cinemas in 1930 as a proxy for historic shops, because we do not have historic data on the location of shops. Historic shops would be a preferred instrument, because one may argue that changing historical buildings for different use (*e.g.* from retail into residential or the other way around) is a costly process and requires changing zoning plans. As a result, current retail outlets tend to be located in buildings where retail was 1930 *even within the same street*.

Historically, most cinemas were small, with one screen only, and were located in shopping streets (those who have survived tend to be relatively large). The buildings containing such cinemas are not very different from the surrounding buildings typically used for shops. One may be concerned that cinemas themselves create footfall, while this does not necessarily imply shopping externalities (as people may only visit the cinema and not visit other shops). Hence, we control for the number of cinemas in the vicinity in 2010.[27] The buildings of the closed cinemas nowadays are frequently used as shops, but also attract other businesses.

The main identifying assumption when relying on long-lagged instruments is that past unobservable characteristics of either stores or locations are uncorrelated to current unobservables (which affect demand). This assumption is often criticised because the locations of cities are determined long ago, *e.g.* due to natural advantages. However, we identify the effect *within* shopping streets, making the identifying assumption more credible.[28] The most obvious threat to identification is that past building and location quality are correlated to shopping externalities. We therefore control for construction period dummies including a dummy whether the shops has been built before 1930. We also include a dummy indicating whether the property is in a historic district to control for the quality of (historic) buildings in the surroundings.

We also construct an alternative instrument based on data of the exact locations of all buildings in 1832, which is available for about 50% of our data. Based on occupational census of land owners, we determine whether a certain building was a shop. We then calculate the number of shops in 1832 in the vicinity. The census from 1832 also provides information on the so-called *Cadastral Income*, which was a tax based on the assessed value of each parcel of the building and the surrounding land. We will therefore control for the value of land in 1832, which should address the issue that past unobservables are correlated to current unobservables. To further control for the fact that a location is in a historically denser part of the street (*e.g.* closer to the city centre), we control for the number of buildings in 1832 in the vicinity.

Hence, our identification strategy exploits that there is a reasonably strong autocorrelation of shop locations over time. This autocorrelation of retail locations is also in line with anecdotal evidence. For example, Amsterdam's current main shopping street, the Kalverstraat, has been an important shopping street for more than 300 years.[29] We emphasise here that the unobserved factors which make shop locations attractive now, are likely very different from the (economic) forces which induced shops to settle at certain locations historically: up to a hundred years ago, shoppers walked to these streets, and shop workers, who were also shop owners, frequently lived above their shops. Nowadays, due to changes in transport technology, shoppers do not walk to the shopping street (but only within), whereas shop workers, who are employees and not shop owners any more, come from far away using modern transport technologies (motor vehicles, public transport, bicycle).

The first stage of our estimation procedure entails:

$$\log s_{ijt} = \tilde{\delta} \log z_{ij} + \tilde{\beta} x_{ijt} + \tilde{\gamma} h_{ij} + \tilde{\varphi}_j + \tilde{\psi}_{i\in j} + \tilde{\theta}_t + \xi_{ijt}, \tag{12}$$

where the $\sim$ refer to first-stage coefficients, z_{ij} is either the number of cinemas in 1930 or the number of shops in 1832 in the vicinity. h_{ij} are control variables related to the historic instruments (*e.g.* number of cinemas in 2010, Cadastral Income in 1832). The second-stage is given by:

$$\log p_{ijt} = \alpha \log \hat{s}_{it} + \beta x_{ijt} + \gamma h_{ij} + \varphi_j + \psi_{i\in j} + \theta_t + \epsilon_{ijt}, \tag{13}$$

where $\log \hat{s}_{ijt} = \{\log \hat{f}_{ijt}, \log \hat{N}_{ijt}\}$ represents the predicted footfall or number of shops from the first stage.

4.2. Vacancies

We will also estimate the effect of log footfal as well as the log number of shops in the vicinity, on whether the shop is vacant, indicated by the dummy variable v_{ijt}. Once again, we will use shopping street fixed effects and exploit the variation in footfall and the probability to be vacant *within* shopping streets. For the same reasons that footfall may be endogenous with respect to rent, it is possible that footfall is endogenous with respect of vacancies. Furthermore, there may be a direct form of reverse causation, because one expects footfall to decrease when vacancies increase. We will use again long-lagged historic instruments to deal with endogeneity issues, as it is impossible that current vacancies directly impact the number of cinemas in 1930 or the number of shops in 1832.

5. Results

The results section is structured as follows. We first discuss the effects of footfall and the number of shops in the shopping street on rents. In the second part, we focus on the effects of footfall and the number of shops on the probability of a shop to be vacant. We take these results together to estimate the effects of footfall and number of shops on rental income. We close this section by discussing the welfare implications and deriving the Pigouvian subsidy.

5.1. Shopping externalities and rents

Table 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 shop, building characteristics, and shopping district fixed effects. The elasticity of footfall with

[27] In the shopping streets we focus on, there are currently 180 cinemas, but in 1930 there were about 60% more (315 cinemas). We control for current cinemas, so variation in cinemas comes from the presence of closed, and likely smaller, cinemas.

[28] Gaigné et al. (2018) argue that for Dutch cities, the amenity distribution within cities has considerably changed. For example, around the 1930s, open water and densely built-up areas were not necessarily considered amenities. It was also before the time when cars became the dominant mode of transport. People usually walked to their working place, and thus commuting distances were very short.

[29] See Fig. B.2 in Appendix B.1 for an example of Amsterdam's city centre.

Table 3
Results for retail rents.

(Dependent variable: log of rent per m²)						
	Footfall			*Number of shops*		
	(1) OLS	(2) OLS	(3) OLS	(4) OLS	(5) OLS	(6) OLS
Footfall *(log)*	0.3737***	0.3181***	0.2609***			
	(0.0149)	(0.0203)	(0.0181)			
Number of shops, <200 m *(log)*				0.1786***	0.2066***	0.1513***
				(0.0214)	(0.0359)	(0.0324)
Size of property *(log)*	0.6376***	0.6267***	0.5929***	0.6323***	0.6283***	0.5938***
	(0.0119)	(0.0121)	(0.0136)	(0.0131)	(0.0126)	(0.0141)
Building – new	0.1341	0.0844	0.0877	0.0980	0.0684	0.0671
	(0.0948)	(0.0790)	(0.0795)	(0.0843)	(0.0805)	(0.0816)
Building – renovated	0.4123***	0.3715***	0.3148***	0.3256***	0.3391***	0.2908***
	(0.1162)	(0.0830)	(0.0842)	(0.0928)	(0.0880)	(0.0847)
Construction year dummies	Yes	Yes	Yes	Yes	Yes	Yes
Location characteristics	No	Yes	Yes	No	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Shopping district fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Shopping street fixed effects	No	Yes	Yes	No	Yes	Yes
Retail sector × chain fixed effects	No	No	Yes	No	No	Yes
Number of observations	4378	4378	4378	4378	4378	4378
R^2	0.682	0.782	0.821	0.604	0.760	0.807

Notes: Footfall is measured as the number of shoppers per day. The construction year dummies are categorised as follows: <1832, 1832–1930, 1931–1950, 1951–1960, 1961–1970, 1971–1980, 1981–1990, 1991–2000, >2000. Location characteristics include whether the property is in a historic district, and the number of geocoded pictures <200 m, religious buildings <200 m, bus stops <200 m, public buildings <200 m, schools, and railway stations <200 m. Robust standard errors are clustered at the postcode and in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

respect to rental price is 0.37. The control variables have plausible signs, with larger and renovated properties being more expensive.[30]

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 bus stop, school or other neighbourhood-specific amenities. A partial solution to this problem is the inclusion of shopping street fixed effects and location characteristics in column (2), which may mitigate this endogeneity issue. This hardly affects the results.

One may still be concerned that rents are partly dependent on the amount of footfall a shop generates – so that reverse causality is an issue. To mitigate this issue, we will include retail sector × chain fixed effects. That is, we include a dummy for whether a retail firm is part of a chain in each retail sector, which should absorb most of the heterogeneity in expected sales between retail firms. The coefficient in column (3) is somewhat smaller and now equals 0.26. Thus, a 10% increase in footfall is associated with a rent increase of 2.6%.

Columns (4)–(6) in Table 3 focuses on another proxy for shopping externalities: the number of shops within 200 m. When using shopping district fixed effects, the elasticity is 0.179 (see column (4)). This elasticity is slightly higher when using shopping street fixed effects in column (5). When controlling for retail sector × chain fixed effects, the elasticity is 0.151. Hence, a 10% increase in the number of shops leads to an increase in rents of 1.5%. The latter elasticity is about 40% lower than the elasticity with respect to footfall. However, the number of shops in the vicinity as a proxy for shopping externalities may be measured with error, *e.g.* because we do not know the exact relevant spatial scale of shopping externalities. We will therefore investigate whether this conclusion still holds when using instrumental variables.

Table 4 further investigates whether the previous results make sense. We first focus on footfall. In column (1) we include retail firm fixed effects, implying that we include dummies for each and every retail chain and independent retail firm. This implies that we compare rent and footfall differences of identical firms within the same shopping street. The results still indicate a sizeable effect of footfall: a 10% increase in footfall is associated with a 1.8% increase in rents. Not surprisingly, because of the large number of fixed effects, the estimate is less precise and only statistically significant at the 5% level. Given the standard error, the estimate is not statistically significantly different from the preferred specification in column (3), Table 3.

In column (2) of Table 4, we control flexibly (using a 5th-order polynomial) for the walking time to the centre of the shopping district, by including a fifth-order polynomial of walking time to the centre. When one-stop shoppers are important, they may access shopping districts on one location (*e.g.* the centre or the edge of a city centre) and then walk to the intended shop in the centre, while passing many other shops (Teulings et al., 2017). On the other hand, controlling for walking time to the centre may also partly absorb shopping externalities. Reassuringly, the footfall coefficient is hardly affected.

One may still be worried that footfall is influenced by the rent that retail firms have to pay (*e.g.* due to rents paid as a function of expected retail revenues or expected generated footfall). In columns (3) and (4) we therefore only include retail firms that are unlikely to contribute (substantially) to local footfall. Column (3) only includes shops that are smaller than 90 m² (the 25th percentile). The estimate is virtually identical to the preferred specification. Also when we only focus on retail firms that are not part of a chain, the estimate is essentially the same. This seems to suggest that the issue of reverse causality is limited. Importantly, it also strongly suggests that the elasticity of rents with respect to footfall does not vary between types of firms, *i.e.* heterogeneity of the estimates is absent, at least when we distinguish between different sizes of firms or whether retail firms are part of a chain. We come back to this issue later on.

We examine endogeneity concerns further in columns (5)–(9) of Table 4 by using historic instruments. Columns (5) and (6) use the number of cinemas in 1930 within 200 m as an instrument for current footfall. With a Kleibergen-Paap *F*-statistic of about 43, the first-stage ap-

[30] Note that the dummy variable indicating whether the property is new is conditional on the construction year, so part of the effect of being in a newer building is absorbed by the construction decade dummies.

Table 4
Footfall and retail rents: extensions.

(Dependent variable: log of rent per m²)	*Retailer fixed effects*	*Walking time to the centre*	*Small shops*	*No chain shops*	*Historic instruments: cinemas in 1930*		*Historic instruments: shops in 1832*		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	OLS	OLS	OLS	OLS	2SLS	2SLS	2SLS	2SLS	2SLS
Footfall *(log)*	0.1820**	0.2032***	0.2556***	0.2435***	0.4563***	0.4348***	0.4034***	0.3740***	0.4799**
	(0.0724)	(0.0189)	(0.0553)	(0.0228)	(0.1061)	(0.0982)	(0.1131)	(0.1105)	(0.2203)
Number of cinemas in 2010, <200 m *(std)*						0.0107			
						(0.0115)			
Cadastral income *(log)*								−0.0177	−0.0213
								(0.0236)	(0.0256)
Cadastral income is zero in 1832								−0.1822	−0.2700
								(0.2659)	(0.3240)
Number of buildings in 1832, <200 m *(std)*									−0.0461
									(0.0868)
Shop characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Location characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Walking time to centre, $f(\cdot)$	No	Yes	No	No	No	No	No	No	No
Shopping street fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Retail firm fixed effects	Yes	No	No	No	No	No	No	No	No
Retail sector × chain fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	4378	4378	987	2932	4378	4378	2496	2496	2496
R^2	0.969	0.826	0.873	0.792					
Kleibergen-Paap *F*-statistic					42.65	48.68	31.46	32.67	17.95
Endogeneity test					4.858	4.426	0.790	0.795	1.153
$\xi^2(2)$ *p*-value					0.0275	0.0354	0.374	0.373	0.283

Notes: Footfall is measured as the number of shoppers per day. Shop characteristics include property size, whether the building is new or renovated and construction year dummies, which are categorised as follows: <1832, 1832–1930, 1931–1950, 1951–1960, 1961–1970, 1971–1980, 1981–1990, 1991–2000, >2000. Location characteristics include whether the property is in a historic district, and the number of geocoded pictures <200 m, religious buildings <200 m, bus stops <200 m, public buildings <200 m, schools, and railway stations <200 m. Robust standard errors are clustered at the postcode and in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

pears to be strong and meaningful: a standard deviation increase in the number of cinemas in 1930 increases current footfall by 12%.[31] The effect of footfall is now somewhat stronger (0.456), which is statistically different from the baseline estimate using a Hausman-test, reported at the bottom of the table.[32] One may be worried that cinemas mostly attract one-stop shoppers. We therefore control for the number of cinemas in 2010 in column (6). It can immediately be seen that the coefficient of footfall is hardly affected. Moreover, the current location of cinemas do not seem to generate higher rents.

Columns (7)–(9) use an alternative set of instruments, by relying on data from the 1832 census. Because these data are only available for about 50% of the Netherlands, our number of observations is lower. Column (7) indicates a strong first stage: a standard deviation increase in the number of shops in 1832 increases footfall by 35%. The first stage is again strong, albeit less strong than when using cinemas in 1930 as an instrument. For this specification, the IV estimate is not statistically significantly higher than the baseline OLS estimate (the endogeneity test has a *p*-value of 0.374). The main advantage of the 1832 data is that we have information on the Cadastral Income, which is a good proxy of the value of property in 1832. Of course, there is a fair share (20%) of missing values, either because land was not (commercially) owned (*e.g.* because it was not yet reclaimed from the sea), or it was missing. We therefore include a dummy indicating whether the Cadastral Income is missing. In any case, controlling for the Cadastral Income does not make any difference, as the Cadastral Income does not seem to be related to current rents of retail properties.[33] Hence, the assumption that unobserved endowments within shopping streets of the past are uncorrelated to current unobserved store and location characteristics, is even more plausible. In the final specification we also control for the number of buildings within 200 m, to make sure that we do not just measure that certain shops are in locations that are denser (*e.g.* city centres). Column (9) shows that it is indeed the locations shops in 1832 that matter for current footfall, as the effect of number of buildings is statistically insignificant. The coefficient also becomes somewhat stronger, but because of the wide confidence bands, it is not statistically significantly different from the baseline OLS estimate.

In Table 5 we repeat the same set of specifications, but now using the other measure for shopping externalities: the number of shops in the vicinity. When using retail firm fixed effects, or when controlling flexibly for the walking time to the centre of the shopping district, the estimates are about 50% lower (respectively columns (1) and (2)). However, when focusing on small stores or when only including chain stores, the elasticities are comparable to the baseline estimates (respectively columns (3) and (4)). Again, the results strongly suggest an absence of between-firm heterogeneity in the elasticity of rent with respect to shopping externalities.

In columns (5)–(9) we use historic instruments. In all specifications, the impact of number of shops is much higher when using IV than when using OLS. In most specifications, endogeneity tests indicate that the estimates using instruments are significantly higher. An important observation is that the IV estimates for the elasticity of number of shops are very comparable to IV estimates for the elasticity of rent with respect to

[31] The first-stage results are reported in Appendix D.1.

[32] We have also re-estimated the model by redefining the instrument into a dummy variable (we then use a binary variable for the proximity to a cinema). With the dummy instrument the IV estimates are somewhat lower, and based on a Hausman test, not statistically significant from the OLS estimates. Consequently, following arguments developed by Sargan (1958), and more recently emphasised by Young (2019), one may prefer the OLS estimates, as these have been more precisely estimated.

[33] Furthermore, the unconditional correlation between log price and log cadastral income per ha is only 0.1505, and the unconditional correlation between these variables without logarithms is essentially zero (and equals −0.01).

Table 5
Number of shops and retail rents: extensions.

(Dependent variable: log of rent per m²)	*Retailer fixed effects*	*Walking time to the centre*	*Small shops*	*No chain shops*	*Historic instruments: cinemas in 1930*		*Historic instruments: shops in 1832*		
	(1) OLS	(2) OLS	(3) OLS	(4) OLS	(5) 2SLS	(6) 2SLS	(7) 2SLS	(8) 2SLS	(9) 2SLS
Number of shops, <200 m *(log)*	0.0740	0.0626**	0.2145***	0.1364***	0.4334***	0.4502***	0.3404***	0.2931***	0.4541
	(0.0860)	(0.0262)	(0.0716)	(0.0372)	(0.1164)	(0.1215)	(0.1219)	(0.1022)	(0.2884)
Number of cinemas in 2010, <200 m *(std)*						−0.0088			
						(0.0122)			
Cadastral income *(log)*								−0.0402	−0.0594
								(0.0284)	(0.0488)
Cadastral income is zero in 1832								−0.1031	−0.2300
								(0.2499)	(0.3711)
Number of buildings in 1832, <200 m *(std)*									−0.0895
									(0.1545)
Shop characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Location characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Walking time to centre, $f(\cdot)$	No	Yes	No	No	No	No	No	No	No
Shopping street fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Retail firm fixed effects	Yes	No	No	No	No	No	No	No	No
Retail sector × chain fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	4378	4378	987	2932	4378	4378	2496	2496	2496
R^2	0.968	0.817	0.871	0.777					
Kleibergen Paap *F*-statistic					48.68	46.64	42.31	62.02	13.88
Endogeneity test					9.380	9.839	4.588	2.350	2.301
$\xi^2(2)$ *p*-value					0.00219	0.00171	0.0322	0.125	0.129

Notes: Footfall is measured as the number of shoppers per day. Shop characteristics include property size, whether the building is new or renovated and construction year dummies, which are categorised as follows: <1832, 1832–1930, 1931–1950, 1951–1960, 1961–1970, 1971–1980, 1981–1990, 1991–2000, >2000. Location characteristics include whether the property is in a historic district, and the number of geocoded pictures <200 m, religious buildings <200 m, bus stops <200 m, public buildings <200 m, schools, and railway stations <200 m. Robust standard errors are clustered at the postcode and in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

footfall. We think that the most likely interpretation of these results is that historic instruments reduce issues related to reverse causality and particularly mitigate measurement error in the number of shops as a proxy for shopping externalities. Historic instruments mitigate this measurement error, because these are arguably uncorrelated to this type of measurement error.[34]

To summarise, we find that both footfall and the number of shops in the vicinity have a positive effect on rents, as expected. When using OLS, we find estimates for footfall of around 0.25, whereas for the number of shops the elasticity is about 0.15. A lower estimate for number of shops makes sense, because it has substantial measurement error (as it does not distinguish between sizes of shops). When using instrumental variables, we find evidence that the estimated effects of footfall and number of shops are roughly the same and range from 0.30–0.45. Note that the latter estimates have larger confidence intervals, in particular when focusing on the effect of number of shops, and do not always differ from the OLS estimates using Hausman tests.

Currently, it is common to measure agglomeration economies using employment at a highly aggregate level, and more specifically focusing on wages. As emphasised by Arzaghi and Henderson (2008), by focusing on rents, and by examining *local* measures of agglomeration that are relevant for the industry examined, *e.g.* number of shops in our context, elasticities may be quite different. Using rents, the study by Koster et al. (2014) reports agglomeration elasticities of employment density that are about 0.10 for retailers (substantially larger than the same elasticity for business services and manufacturing which is estimated to be about 0.04). Their estimate is still far below our estimated agglomeration elasticities of number of shops (as well as footfall) implying that employment density is a poor proxy for agglomeration for retail compared to retail specific measures (*i.e.* number of shops, footfall) that are used in the current paper.

5.2. Shopping externalities and vacancy rates

In Table 6, we report the results for the incidence of a shop being vacant as a function of footfall using a Linear Probability Model (LPM) based on a similar set of specifications as in the previous subsection. Column (1) is a naïve regression of a dummy variable, which indicates if a shop is vacant on log footfall, the log footprint area of the building, construction year dummies, shopping district and year fixed effects. The coefficient implies that when footfall increases by 10%, the vacancy rate decreases by 0.31 percentage points – about 5% of the mean vacancy rate.

The estimated effect is slightly higher when we include location characteristics and shopping street fixed effects in column (2). This preferred OLS estimate indicates that a 10% increase in footfall leads to a decrease in the vacancy rate of 0.35 percentage points. Given an average vacancy rate of 6.1%, this implies a reduction in vacancy rates of 5.7%.

Column (3) investigates whether the effects of footfall on the incidence of being vacant is different. When focusing on building footprints equal to or smaller than 25 m², the results are similar. Also when controlling flexibly for walking time to the centre, the results are not substantially affected. Using long-lagged instruments in columns (5)–(9) of Table 6 should address any remaining endogeneity issues. In column (5) we use cinemas in 1930 as an instrument for current footfall. In line with the analysis for rents, the coefficient becomes stronger. A 10% increase in footfall is now associated with a decrease in the vacancy rate

[34] It seems unlikely that the spatial relationships between cinemas in 1930 or the number of shops in 1832 and the number of shops in the vicinity is the same as the spatial relationship between shopping externalities and the current number of shops. If there would be a strong correlation, the instruments are unlikely to address measurement error. Nevertheless, one may argue that the measurement error may be correlated when choosing the same distance cut-off (which is 200 m). We therefore have experimented with using instruments based on different thresholds leading to nearly identical outcomes.

Table 6
Footfall and vacancies.

(Dependent variable: shop is vacant)	*District fixed effects*	*Shopping street fixed effects*	*Small shops*	*Walking time to the centre*	*Historic instruments: cinemas in 1930*		*Historic instruments: shops in 1832*		
	(1) OLS	(2) OLS	(3) OLS	(4) OLS	(5) 2SLS	(6) 2SLS	(7) 2SLS	(8) 2SLS	(9) 2SLS
Footfall *(log)*	−0.0314*** (0.0011)	−0.0352*** (0.0016)	−0.0302*** (0.0025)	−0.0326*** (0.0017)	−0.0710*** (0.0098)	−0.0640*** (0.0091)	−0.0290*** (0.0067)	−0.0328*** (0.0066)	−0.0146 (0.0138)
Number of cinemas in 2010, <200 m *(std)*						−0.0036*** (0.0011)			
Cadastral income *(log)*								0.0021 (0.0018)	0.0029 (0.0018)
Cadastral income is zero in 1832								0.0147 (0.0239)	0.0255 (0.0233)
Number of buildings in 1832, <200 m *(std)*									−0.0109 (0.0069)
Shop characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Location characteristics	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Walking time to centre, $f(\cdot)$	No	No	No	Yes	No	No	No	No	No
Shopping district fixed effects	Yes	No	No	No	No	No	No	No	No
Shopping street fixed effects	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Retail firm fixed effects	Yes	No	No	No	No	No	No	No	No
Retail sector × chain fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	410,544	410,544	108,073	410,064	410,544	410,544	410,544	218,271	218,271
R^2	0.0276	0.0442	0.0617	0.0443					
Kleibergen Paap F-statistic					80.24	84.20	129.6	145.1	43.87
Endogeneity test					15.93	11.27	0.803	4.90e-05	1.901
$\xi^2(2)$ p-value					6.58e-05	0.000786	0.370	0.994	0.168

Notes: Footfall is measured as the number of shoppers per day. Shop characteristics include property size, whether the building is new or renovated and construction year dummies, which are categorised as follows: <1832, 1832–1930, 1931–1950, 1951–1960, 1961–1970, 1971–1980, 1981–1990, 1991–2000, >2000. Location characteristics include whether the property is in a historic district, and the number of geocoded pictures <200 m, religious buildings <200 m, bus stops <200 m, public buildings <200 m, schools, and railway stations <200 m. Robust standard errors are clustered at the postcode and in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.10$

of 0.7 percentage points. The estimate is statistically different from the comparable OLS estimate, as indicated by the endogeneity test. Controlling for the current number of cinemas does not change this result (see column (6)).

In columns (7)–(9) of Table 6, we use an alternative instrument – the number of shops in 1832 within 200 m. The coefficient is now much closer to the baseline OLS estimate. A 10% increase in footfall is associated with a 0.29 percentage points increase in the vacancy rate. This estimate is slightly higher when we control for the Cadastral Income and somewhat lower when we control for the number of buildings in 1832. Although the latter estimate is not statistically significant at conventional levels, it is not significantly different from the OLS estimate either.

We repeat the same set of specifications, but now using the number of shops within 200 m as a proxy for shopping externalities. Table 7 reports the results. Columns (1)–(4) report OLS estimates. The preferred estimate in column (2) shows that a 10% increase in the number of shops is associated with an increase of 0.5 percentage points in the vacancy rate. This effect is close to the estimate for footfall.

In the last 5 columns of Table 7 we use historic instruments. The estimates based on cinemas in 1930 show similar results to the OLS estimates (see columns (5) and (6). When using shops in 1832 the coefficients are somewhat lower, although the difference is hardly statistically significant.

All in all, we find that both footfall and number of shops in the vicinity have a negative effect on the vacancy rate, in line with expectations. We find again evidence that the estimated coefficients for footfall and number of shops are similar and range from 0.03–0.05.

5.3. Shopping externalities and rental income

In Section 2.1, we argued that shopping externalities are expected to capitalise into rental incomes of shop owners. Rental incomes are defined as the annual 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 5.1 and 5.2, Table 8 provides the estimates for the effect of shopping externalities on rental income. Following Eq. (2), we calculate this effect assuming different values of κ:

$$\frac{\partial \log I_{ij}}{\partial \log s_{ij}} = \frac{\partial \log p_{ij}}{\partial \log s_{ij}} - (1+\kappa)\frac{\partial v_{ij}}{\partial \log s_{ij}}. \tag{14}$$

Recall that κ is a positive parameter that defines the relationship between advertising cost and rental price. We assume that κ is equal to 0.426, based on the costs of letting commercial space, which is about 17.5% of the yearly rental value in the Netherlands (Leurs, 2017).[35] We also will estimate specifications where we either assume that fees are zero ($\kappa = 0$) or that fees are several times higher and equal to 50% of the annual rental value, so that $\kappa = 1.2185$.

Following Section 2.2, Table 8 reports the estimated elasticities for the preferred specifications.[36] In the first three columns we focus on the elasticity of rental income with respect to footfall, denoted by $\varepsilon_{I,f}$. Column (1) reports the estimated elasticity based on the preferred OLS specifications. We find elasticities of around 0.3, depending on the value of κ we assume. However, it can be seen that using wildly different values for advertising costs κ makes little difference for the estimated elasticity. When we concentrate on the 2SLS estimates using historic instruments, the elasticity of rental income with respect to footfall is

[35] From a small subset of the observations in *Strabo*, we know that the average contract length is 6.77 years, whereas the average vacancy rate is 0.061. Hence, $\kappa = 0.175/(6.77 \times 0.061) = 0.426$.

[36] For footfall, these are reported in column (3) of Table 3, column (2) of Table 6, columns (6) and (9) of Table 4. For the number of shops, those are based on column (6) of Table 3, column (2) of Table 7, columns (6) and (9) of Table 5.

Table 7
Number of shops and vacancies.

(Dependent variable: shop is vacant)	*District fixed effects*	*Shopping street fixed effects*	*Small shops*	*Walking time to the centre*	*Historic instruments: cinemas in 1930*		*Historic instruments: shops in 1832*		
	(1) OLS	(2) OLS	(3) OLS	(4) OLS	(5) 2SLS	(6) 2SLS	(7) 2SLS	(8) 2SLS	(9) 2SLS
Number of shops, <200 m *(log)*	−0.0382***	−0.0505***	−0.0382***	−0.0420***	−0.0579***	−0.0540***	−0.0332***	−0.0358***	−0.0135
	(0.0024)	(0.0032)	(0.0055)	(0.0036)	(0.0078)	(0.0078)	(0.0080)	(0.0075)	(0.0129)
Number of cinemas in 2010, <200 m *(std)*						−0.0025**			
						(0.0011)			
Cadastral income *(log)*								0.0034**	0.0034**
								(0.0016)	(0.0016)
Cadastral income is zero in 1832								0.0277	0.0317
								(0.0210)	(0.0211)
Number of buildings in 1832, <200 m									−0.0001**
									(0.0000)
Shop characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Location characteristics	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Walking time to centre, $f(\cdot)$	No	No	No	Yes	No	No	No	No	No
Shopping district fixed effects	Yes	No	No	No	No	No	No	No	No
Shopping street fixed effects	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Retail firm fixed effects	Yes	No	No	No	No	No	No	No	No
Retail sector × chain fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	410,544	410,544	108,073	410,064	410,544	410,544	410,544	218,271	218,271
R^2	0.0203	0.0404	0.0589	0.0406					
Kleibergen Paap *F*-statistic					282.8	284.8	299.7	367.6	97.74
Endogeneity test					0.979	0.240	4.252	3.486	9.017
$\xi^2(2)$ *p*-value					0.322	0.624	0.0392	0.0619	0.00268

Notes: Footfall is measured as the number of shoppers per day. Shop characteristics include property size, whether the building is new or renovated and construction year dummies, which are categorised as follows: <1832, 1832–1930, 1931–1950, 1951–1960, 1961–1970, 1971–1980, 1981–1990, 1991–2000, >2000. Location characteristics include whether the property is in a historic district, and the number of geocoded pictures <200 m, religious buildings <200 m, bus stops <200 m, public buildings <200 m, schools, and railway stations <200 m. Robust standard errors are clustered at the postcode and in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

Table 8
The elasticity of rental income with respect to shopping externalities.

	Footfall			*Number of shops*		
	Baseline specification (1) OLS	*Cinemas in 1930* (2) 2SLS	*Shops in 1832* (3) 2SLS	*Baseline specification* (4) OLS	*Cinemas in 1930* (5) 2SLS	*Shops in 1832* (6) 2SLS
$\kappa = 0.426$	0.311***	0.526***	0.528***	0.223***	0.527***	0.559**
	(0.0160)	(0.0863)	(0.188)	(0.0287)	(0.108)	(0.276)
$\kappa = 0.000$	0.296***	0.499***	0.523***	0.202***	0.504***	0.554**
	(0.0159)	(0.0859)	(0.188)	(0.0285)	(0.107)	(0.276)
$\kappa = 1.218$	0.339***	0.577***	0.536***	0.263***	0.570***	0.566**
	(0.0162)	(0.0875)	(0.189)	(0.0293)	(0.110)	(0.275)

Notes: This is a Zellner-style seemingly unrelated regression, where error terms are allowed to be correlated across the rent and vacancy rate equation. We include shop and location characteristics, as well as shopping street fixed effects, retail sector × branch fixed effects and year fixed effects. Robust standard errors are clustered at the postcode and in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

considerably higher and around 0.5, independently of the instruments we use.

Let us now focus on the alternative measure of shopping externalities: the elasticity of rental income with respect to the number of shops within 200 m. Table 8 uses the estimated effects of the number of shops in each shopping street on rents and vacancies. The estimates obtained from the OLS specifications are again lower and around 0.22, while the estimates based on historic instruments are around 0.5. Hence, $\varepsilon_{I,f}$ seems to be roughly the same as $\varepsilon_{I,N}$.

Let us now calculate the marginal effect of footfall on rental income in absolute terms, using Table 8. 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 rental income per m^2 is then approximately € 0.000045.[37] Consequently, the monetary benefit of one additional pedestrian passing a shop with an average size of about 200 m^2 is estimated to be about € 0.009.[38] In the same way, it can be shown that the monetary benefit of one additional shop is about € 0.65 per m^2, hence the marginal benefit for a shop with an average size of about 200 m^2 is about € 130. Hence, the share of retail firms' rental expenditure spent on agglomeration is, on average, about 0.43. Such a high share is in line with Koster et al. (2014) and is conform intuition as we focus on retail firms that choose to locate within (expensive) shopping streets, because of positive shopping externalities.

5.4. Welfare analysis

We have shown that shopping externalities are important and argued that due to fragmented ownership of shops, it is implausible that shopping externalities are internalised.[39]

[37] This number is the product of the log footfall coefficient of 0.5 and the average annual rental income per m^2 (€ 309 per m^2) divided by the product of the mean footfall (13,328), multiplied by 5 (because footfall on Saturdays is one fifth of weekly footfall) and by the number of weeks in a year.

[38] 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% of those who enter the shop also make a purchase and the profit per purchase is equal to € 3. The marginal profit of footfall for a shop is then equal to $0.01 \times 0.25 \times 3 =$ € 0.0075.

[39] In Appendix D.3 we investigate further whether internalisation occurs by exploiting ancillary information on whether store owners possess multiple properties in a street (which applies to 18% of the shops). We show that shops owned by multiple store owners do not have a different footfall elasticity.

We will now examine policies that subsidise store owners. Our focus is on local agglomeration economies. Local variation of agglomeration capitalisation into wages will be assumed to be absent. This is reasonable to assume, because differences in commuting time between local shops are essentially zero. Consequently, localised labour supply will be assumed to be perfectly elastic, and all productivity gains are reflected in rental income.

We aim to make statements about welfare improvements that are possible using subsidies, but we do not make any claims about the magnitude of the welfare improvement. The latter relies, among other things, on assumptions about whether the local (as well as total) demand for shopping is elastic or inelastic. Our argument for subsidies is that, in contrast to shopping malls, there is no owner/manager who internalises the shopping externalities and who will choose an optimal amount and composition of shops.

We have demonstrated that the elasticity of rental income with respect to number of shops $\varepsilon_{I,N}$ is about 0.5 (see Table 8). Given this elasticity, we calculate the optimal subsidy for store owners, which is equal to the marginal external benefit of one additional shop. Eq. (6) implies that the optimal subsidy to store owners is about 50% of rental income, which is substantial. Given an average rental income of about € 50 thousand, the optimal annual subsidy per shop is about € 25 thousand, on average (using the median rental income of about € 34 thousand, provides an optimal annual subsidy per shop of about € 17 thousand). Such a number is not implausibly high, at least compared to the millions of shoppers who pass a shop each year (average annual footfall is about 3.5 million). Optimal subsidies vary strongly between different shopping streets. Larger shopping streets must receive higher levels of subsidies, because there are more shops and because rental income levels are higher in these streets. In our data, it turns out that the 25 largest shopping streets (of the population of about thousand shopping streets) would receive about 25% of all national subsidies.

We are also interested in the optimal subsidy to footfall. We use the right-hand side of Eq. (9), which equals $N \cdot I \cdot \varepsilon_{I,f}/f$ given the assumption of homogeneous retail firms. The elasticity of rental income with respect to footfall $\varepsilon_{I,f}$ is estimated to be about 0.5 (see Table 8). The median of $N \cdot I/f$ is about 1.56 (the mean, excluding extreme outliers, is about 30% higher). Consequently, the marginal external benefit of one shopper is about € 0.78. Although it is difficult to judge whether such a number makes sense, we note that it is low compared to typical subsidies to public transport or parking, which are usually justified by policy makers as these subsidies are argued to induce more shoppers.

At this point, we would like to point out four caveats. The first caveat is that our estimates of the optimal subsidies to store owners and to footfall depend on the assumption with respect to the number of shops passed by a typical shopper. In the calculation of the optimal subsidy, we have assumed that shops benefit from other shops within 200 m. In the calculations of the optimal footfall subsidy, we have assumed that a shopper passes all other shops within 200 m. We note that with other thresholds the coefficients and resulting subsidies are similar, but not identical.[40]

The second caveat is that the subsidy is only optimal in the first-best equilibrium, hence it assumes the absence of other externalities. In this context, it is important to emphasise the presence of negative crowding within shopping streets as well as negative, but under-priced, travel externalities of shoppers who travel by car towards shopping streets causing road congestion. We believe that the extent of negative crowding externalities within shopping streets are present but usually small, except for special days (*e.g.* Black Friday, the weekend before Christmas), which suggest that the subsidies should be somewhat smaller than suggested above. Further, it is a priori not clear whether a higher concentration of shops stimulated by policy reduces or amplifies road congestion. Given that the proportion of shoppers who visit multiple shops is high, a higher concentration of shops induces shoppers to make only one trip by car, so that it is possible that such policy would reduce total distance travelled by car, and therefore road congestion. However, if shops are more concentrated, people may have to drive further to the nearest shopping street, which may lead to more congestion. Hence, whether the subsidy calculated above has to be somewhat adjusted upwards or downwards is not so clear.

A third caveat is that we ignored any effects of new shops and footfall on product prices. These prices will be fixed for retail firms that are part of a chain (about one third of our observations), but may adjust for non-chain firms. The welfare effects of adjustments in product prices are expected to be included in the rent estimates (*i.e.* retail firms can ask higher product prices will be willing to pay higher rents), except if market power in the product market is strongly correlated to footfall or number of shops.

Finally, in these calculations, we assumed that $\varepsilon_{I,N}$ and $\varepsilon_{I,f}$ are constant, and therefore do not differ between firms located within shopping streets. This may not be very realistic when firms differ in the extent that they benefit from the presence of other firms. This has important repercussions for the introduction of a non-marginal subsidy, as it implicitly assumes that $\varepsilon_{I,N}$ and $\varepsilon_{I,f}$ do not vary with the level of the subsidy. Arguably, if this assumption does not hold, the subsidy induces sorting of retail firms: firms with different underlying parameters enter (or leave) the shopping street, such that the values of $\varepsilon_{I,N}$ and $\varepsilon_{I,f}$ will change. To examine this further, in the next section, we allow the effects of footfall and number of shops on rents to be retail firm-specific. We provide, however, evidence that between-retail-firms heterogeneity is of limited importance. This suggests that due to the introduction of a non-marginal subsidy, the type of firms in shopping streets, at least as measured by $\varepsilon_{I,N}$ and $\varepsilon_{I,f}$, will not substantially change, which increases the external validity of the welfare-optimal subsidy estimates.

So far, we have examined the use of subsidies to store owners as a means of internalising shopping externalities. As an alternative to subsidies, policymakers may also use policies which aim to influence the number (and even the composition) of shops directly. In particular, zoning is likely to be a welfare-increasing instrument, but only if the regulator has sufficient information about what should be the optimal size of a shopping district. Note that in absence of such information, zoning regulations restricting shopping areas to grow (*e.g.*, at the expense of residential housing) is unlikely to be optimal because of the magnitude of shopping externalities.

Until this point, we have ignored the effects of the subsidies on shopping demand. In the absence of externalities other than shopping externalities (*e.g.* crowding and road congestion externalities), reductions in demand elsewhere would not affect our main welfare arguments, but it would reduce the magnitude of welfare gains if these welfare benefits were calculated only at the locations which receive subsidies.[41] In this context, inelastic total demand seems a reasonable assumption before the rise of online shopping, as shown by Rosenthal and Ross (2010). In this case, welfare gains would still arise from spatial concentration of retail, because of reduced transport cost associated with shopping. On the other hand, nowadays it is plausible that total demand for *street* shopping is elastic, as it strongly competes with online shopping. Moreover, tourism has become more important for shopping in city centres. Because spatially-concentrated retail clusters nowadays attract tourism, it is plausible that demand becomes even more elastic. Consequently, productivity and welfare gains of subsidies, at the level of the city, might be larger.

[40] For example, when choosing 250 m as threshold, the external benefit of one shopper is about € 1, while when setting the threshold to 100 m, the external benefit is € 0.60. In Appendix D.3 we also estimate the effects of number of shops on rents for different thresholds, leading to very similar results for $\varepsilon_{I,N}$.

[41] Note we refrain from any exercise which tries to evaluate the welfare effects of local subsidies, because one needs a full-fledged structural equilibrium model to say anything about the net welfare gains of such a subsidy.

6. Extensions and sensitivity analysis

6.1. Heterogeneity

Shopping externalities are likely heterogeneous between different stores. Hence, conclusions regarding non-marginal policies highly depend on whether the willingness to pay for footfall and for number of shops vary between different types of shops. To address this issue, we re-estimate the hedonic price function, but allow the parameters to vary by retail characteristics.[42]

Hence, in line with Eq. (11), we have:

$$\log p_{ijt} = \alpha_i \log s_{ijt} + \beta_i x_{ijt} + \varphi_j + \psi_{i\in k} + \theta_t + \epsilon_{ijt}. \tag{15}$$

Note that the parameters α_i, which captures shopping externalities s_{ijt}, and β_i, which captures the effect of control variables x_{ijt}, are both retail firm-specific. Identifying these parameters is not possible without additional restrictions, as estimating α_i and β_i separately for each firm would imply that we have have no degrees of freedom. We therefore use a semi-parametric Kernel-smoothing procedure, where α_i and β_i are allowed to depend, in a flexible way, on firm characteristics z_i. In Appendix C.1 we outline this procedure in detail, explain how to determine the smoothing parameter and how we deal with instrumental variables using a so-called 'control function' approach. Given $\hat{\alpha}_i$, estimated for footfall and number of shops, we recover $\hat{\varepsilon}_{I,f}(i)$ and $\hat{\varepsilon}_{I,N}(i)$.

Following Bajari and Kahn (2005) and Koster et al. (2014), in a second step, we aim to explain heterogeneity by relating $\hat{\varepsilon}_{I,f}(i)$ and $\hat{\varepsilon}_{I,N}(i)$ to firm characteristics. We rely here on straightforward linear regression techniques by estimating:

$$\{\hat{\varepsilon}_{I,f}(i), \hat{\varepsilon}_{I,N}(i)\} = \eta z_{it} + v_{it}, \tag{16}$$

where η are parameters to be estimated and v_{it} is an error term. To obtain standard errors we cluster bootstrap the standard errors in this two-step procedure.

We report results with respect to the first step of the estimation procedure in Fig. 2. We report distributions of $\hat{\varepsilon}_{I,f}(i)$ and $\hat{\varepsilon}_{I,N}(i)$. Fig. 2(a) shows that the distribution of $\varepsilon_{I,f}$ ranges from 0.25 to 0.35. When instrumenting for footfall with cinemas in 1930, the estimates have a larger spread and range from 0.27 to 0.67, but most of the estimates are still between 0.39 and 0.55 (see Fig. 2(b)). In Fig. 2(c) we find that the distribution of $\varepsilon_{I,N}$ ranges from 0.16 to 0.26. There is also limited heterogeneity in the estimates where we instrument for the number of shops (see Fig. 2(d)): the estimates range from 0.46 to 0.56. Again, we do not find evidence of substantial heterogeneity.

In Table 9 we aim to explain heterogeneity using retail firm characteristics. The constant shows that the annual estimates are close to the baseline results.[43] We find limited evidence in column (1) that stores selling daily items, cafés and restaurants, and services have a lower preference for footfall, although the estimates are statistically insignificant when we instrument. On the other hand, when we use instruments, we find that stores that are part of a retail chain seem to have a somewhat higher preference for footfall.

All in all, we find limited evidence that heterogeneity is very important. This may seem surprising, but we note that the focus of this paper is on designated shopping streets. Hence, retail firms that do not have a strong preference for footfall will not locate in those areas. For example, firms like *IKEA* or *WalMart* attract usually one-stop shoppers, but usually do not locate in shopping streets or malls. The lack of heterogeneity strongly suggest that the results derived in the welfare analysis also hold for non-marginal policy changes.

6.2. Sensitivity analysis

We subject our results to several robustness checks. First, in Appendix D.2 we consider to what extent omitted variables bias our baseline estimates for rents, by applying Oster's (2019) methodology to provide bias-corrected estimates. The resulting coefficients appear to be very close to the reported baseline estimates.

We report many more robustness checks. In Appendix D.3 we focus on rents, while in Appendix D.4 we report results for vacancies.

First, we consider an alternative identification strategy where we compare shops near shopping street intersections. Hence, rather than identifying the effects *within* shopping streets, we now compare differences in footfall and number of shops *between* shopping streets that intersect. Then, we only keep observations within 250 m of an intersection and include fixed effects for each shopping street intersection. By using the spatial variation in footfall between intersecting streets, we control for unobserved locational endowments that attract both shoppers and retail firms (*e.g.* free parking etc.). We show that the results are very much comparable to the baseline specifications using shopping street fixed effects.

Second, one may argue that different real estate agents have different bargaining power or sales strategies, which may lead to different observed rents and/or unreported incentives. However, when including real estate agent fixed effects the results are essentially unaffected.

Third,use observations of rents and vacancies in order to get information about rental income, but one may also consider to use sales price directly. We only have 110 sales price observations, so we cannot use detailed fixed effects or instrument for shopping externalities. Nevertheless, the point estimates are in line with the baseline results using rents.

We further show the effects of shopping externalities on log rents using quadratic specifications of log footfall to allow for non-linear effects of the logarithm of footfall. In OLS specifications we find evidence of a positive non-linear effect, but when we instrument, this effect is highly statistically insignificant. We also show that the results are robust when using different thresholds in counting the number of shops in the vicinity (*e.g.* 100 m, 200 m, 250 m).

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. In Appendix D.3, we test whether the effect of footfall on retail rents is capitalised differently in properties that belong to store owners who possess multiple rental properties in the same shopping street, which we label as multi-property owners. For multi-property owners, the externality seems to capitalise in rents in the same way as for single property owners. To further investigate the issue of internalisation, we investigate whether shopping externalities capitalise differently in rents when the owner is a private investor. We do not find evidence for this scenario. Furthermore, using information on area-based retail associations from Menger (2014), we also do not find evidence that externalities are internalised by retail associations.

We also investigate the robustness of the estimates of the impact of shopping externalities on vacancy rates in Appendix D.4. We show that results are similar when we use the alternative identification strategy with shopping street intersection fixed effects. Moreover, it appears that using a Probit model instead of a Linear Probability Model, the estimates of the effect of shopping externalities on vacancy rates are very similar.

There might be alternative explanations that explain the effect of footfall on retail vacancy rates. One such explanation is that the effect of

[42] We will maintain the assumption that the effect on the vacancy rate is homogeneous (as we do not observe retail characteristics for vacant properties). Because of the small contribution of the effect of footfall and number of shops through changes in vacancy rates in $\hat{\varepsilon}_{I,f}(i)$ and $\hat{\varepsilon}_{I,N}(i)$ (see Table 8), we do not consider this as a major drawback.

[43] We do not report results using shops in 1832 as instruments. Because our sample is then much smaller, it appears that we have too limited power to estimate these regressions and obtain meaningful results.

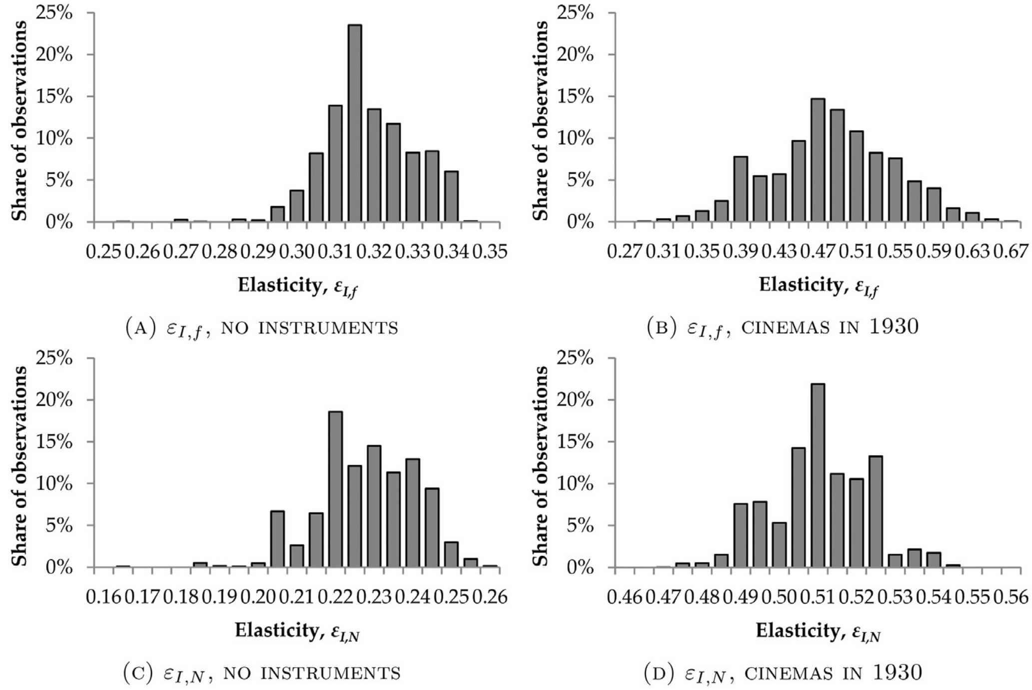


Fig. 2. Results of the first step.

Table 9
Explaining variation in $\hat{\varepsilon}_{I,f}$ and $\hat{\varepsilon}_{I,N}$.

Dependent variable:	$\hat{\varepsilon}_{I,f}$ (1) OLS	$\hat{\varepsilon}_{I,N}$ (2) OLS	$\hat{\varepsilon}_{I,f}$ (3) OLS	$\hat{\varepsilon}_{I,N}$ (4) OLS
Instruments used in first stage:			*Cinemas in 1930*	
Part of retail chain	0.0083	0.0026	0.0618*	0.0127
	(0.0069)	(0.0112)	(0.0343)	(0.0157)
Small shop	−0.0023	0.0079	−0.0639**	−0.0143
	(0.0062)	(0.0098)	(0.0311)	(0.0117)
Large shop	−0.0036	−0.0054	0.033	0.0034
	(0.0079)	(0.0118)	(0.0316)	(0.0134)
Food store	−0.0055	0.0203	−0.0172	0.0011
	(0.0114)	(0.0137)	(0.0441)	(0.0180)
Daily items	−0.0142**	−0.0015	−0.0204	−0.0021
	(0.0059)	(0.0070)	(0.0224)	(0.0088)
Sports and leisure	−0.0142	0.0225*	0.0539	0.0169
	(0.0112)	(0.0126)	(0.0431)	(0.0166)
Living	−0.0153	0.0104	−0.0175	0.0028
	(0.0123)	(0.0172)	(0.041)	(0.0161)
Other retail stores	−0.0130	0.0219	0.0292	0.0116
	(0.0132)	(0.0160)	(0.0586)	(0.0187)
Cafés, restaurants, leisure	−0.0228*	−0.0007	−0.0604	−0.0005
	(0.0132)	(0.023)	(0.0461)	(0.0175)
Specialised stores	−0.0126	0.0207	−0.0192	0.0000
	(0.0135)	(0.0144)	(0.0475)	(0.0172)
Services	−0.0251**	0.0155	−0.0535	−0.0122
	(0.0128)	(0.0138)	(0.0483)	(0.0185)
Constant	0.3246***	0.1884***	0.5107***	0.5241***
	(0.0249)	(0.0386)	(0.104)	(0.1395)
Number of observations	4378	4378	4378	4378
R^2	0.743	0.718	0.901	0.938

Notes: Clothing stores are the omitted category. Bootstrapped standard errors (100 replication) are clustered at the postcode and in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

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 (see *e.g.* Teulings et al., 2017). 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. In order to test this scenario, we regress the dummy for vacant shops on shopping externalities and on the interaction term between shopping externalities and a dummy variable for the recent boom (and bust) period of the Dutch economy. Our results show that the effect of footfall on vacancy rates is significantly 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 confirm 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).

Finally, in Appendix D.4 we make a distinction between the effects of shopping externalities on short-term (less than 1 year) and long-term (at least 3 years) vacancies. This could be interpreted as the difference between 'frictional' and 'structural' vacancies. We do not find systematic evidence that shopping externalities do impact short-term and long-term vacancies differently.

7. Conclusions

The findings of this paper add to our understanding of retail clustering and shopping externalities. We use a novel proxy for shopping externalities – footfall – together with a more standard proxy – the number of shops in the vicinity. Economic theory indicates that *(i)* shop rents depend positively on footfall and the number of shops in the vicinity, and *(ii)* vacancies depend negatively on footfall. Hence, the effect of shopping externalities on rental income – the shop rent multiplied with the share of the time that the shop is occupied – is positive.

Our empirical estimates for the main shopping streets of the Netherlands show that the effects of footfall on retail rents and vacancies are substantial. Shopping externalities are therefore crucial to retail location choices, as higher footfall leads to higher rental income, with an elasticity in the range 0.25–0.50. Such a high elasticity is consistent with the notion that the main reason for shops to cluster is the presence of positive shopping externalities. We use instrumental variables based on retail locations in 1930 (and even 1832), which address a range of endogeneity issues. When instrumenting, the elasticity of rental income with respect to footfall and the elasticity of rental income with respect number of shops in the vicinity are about the same.

We find limited evidence that the elasticities are very heterogeneous, which underlines that retail property markets are highly competitive and that shopping streets attract retail firms that benefit in a similar way from shopping externalities. The type of firms attracted to locate in expensive shopping streets is highly selective (*e.g.*, 30% of shops are clothing stores), and the high rents dissuade retail firms that cannot reap the benefits of shopping externalities.

We demonstrate that it is implausible that the positive externalities in shopping streets are internalised due to the high fragmented ownership of shops, in contrast to shopping malls. To formulate our policy recommendations, we derive *(i)* the shopâs marginal benefit of a pedestrian passing by, *(ii)* the optimal subsidy to store owners as an incentive to provide more retail space in an existing shopping street.

We have shown that shopping externalities are important – the shop's marginal benefit of a passing pedestrian is about € 0.009. The optimal subsidy to store owners is about 25% of rental expenditure (or even higher), about € 12,500 per year. We are aware that explicit subsidies to private firms are controversial and may even be illegal. However, current policy practices in many cities around the world to pedestrianise certain streets in attractive city centres and the provision of subsidised public transport or subsidies to short-term parking space close to shopping streets are an implicit subsidy to enhance shopping externalities. From the perspective of the retail market, those policies seem welfare improving.

Appendix A

A1. Theoretical model

Let us introduce a search model of a shopping street with two types of homogeneous agents. Store owners that possess properties and retail firms, which rent properties from the store owners. Store owners with vacant properties and retail firms searching for space have to search for each other. Store 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, which possess one shop each, which they aim to rent out to a retail firm for rent p. For simplicity, the revenue of a shop is equal to footfall in the street. For now, we assume that the number of shops 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 searching for shop space 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: the number of vacant shops v times advertising expenditure e per property, ev. Thus, $m = m(ev)$. Vacant shops 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 costs are 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 $p(1 - v) - cv$.

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. Store owners with vacancies choose their advertising expenditure conditional on the advertising expenditure of other store 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:

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

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

$$rV = -c(e) + \frac{m(ev)}{v}(R - V), \tag{A.2}$$

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

$$rR = p + \delta(V - R). \tag{A.3}$$

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

$$rS = f - p - \delta(S - Q). \tag{A.4}$$

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

$$rQ = -z(\eta) + \lambda(S - Q). \tag{A.5}$$

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

$$(1 - \beta)S = \beta(R - V). \tag{A.6}$$

These four equations, combined with the first-order condition of (A.2) that the present-discounted value of expected profits of a vacant shop is maximised with respect to advertising expenditure $c(e)$, imply that in equilibrium, p, v and e are determined by the following three equations:

$$p = \frac{f(1 - \beta)\big(v(r + \delta) + m(ev)\big) - (r + \delta)v\beta c(e)}{(1 - \beta)m(ev) + v(r + \delta)}, \tag{A.7}$$

$$v = 1 - \frac{m(ev)}{\delta}, \tag{A.8}$$

$$c'(e) = \frac{(1-\beta)(f + c(e))m(ev)}{erv + e\delta\big(1 - (1 - v)\beta\big)}. \tag{A.9}$$

We are interested in the effects of footfall on prices and vacancy rates. Using (A.7), 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 1. *In equilibrium, (i) shop price depends positively on footfall and (ii) the number of vacancies depends negatively on footfall.*

Proof. We first derive V, R, S and p by solving the system of Eqs. (A.2)–(A.4) and (A.6). This leads to:

$$V = \frac{(1-\beta)mf - (r+\delta)cv}{r\big((1-\beta)m + (r+\delta)v\big)}, \tag{A.10}$$

$$R = \frac{f(m + rv)(1-\beta) - (r\beta + \delta)cv}{r\big((1-\beta)m + (r+\delta)v\big)}, \tag{A.11}$$

$$S = \frac{(c+f)v\beta}{m(1-\beta) + v(r+\delta)}, \tag{A.12}$$

$$p = \frac{f(1-\beta)(v(r+\delta) + m) - (r+\delta)v\beta c}{(1-\beta)m + v(r+\delta)}. \tag{A.13}$$

Note that $m = m(ev)$ and $c = c(e)$. First, we are interested in the effect of footfall on rents, so $\mathrm{d}p/\mathrm{d}f$. We then use Eqs. (A.7)–(A.9), and using implicit differentiation. According to Cramer's rule, $\mathrm{d}p/\mathrm{d}f = \det(Z_p)/\det(Z)$, where:

$$Z = \begin{bmatrix} 1 & \frac{(1-v)(1-\beta)(r+\delta)(f+c)\beta\delta v}{e\left(rv+\delta\left(1-\beta(1-v)\right)\right)^2} & 0 \\ 0 & \frac{m}{\delta e} & 1 + \frac{m}{\delta v} \\ 0 & -\frac{(1-\beta)mc'}{erv+e\delta\left(1-\beta(1-v)\right)} + c'' & -\frac{(1-\beta)^2(f+c)\delta m}{ev\left(rv+\delta\left(1-\beta(1-v)\right)\right)^2} \end{bmatrix} \tag{A.14}$$

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:

$$z = \begin{bmatrix} \frac{(1-\beta)(rv+\delta)}{rv+\delta\left(1-\beta(1-v)\right)} \\ 0 \\ \frac{(1-\beta)m(e)}{erv+e\delta\left(1-\beta(1-v)\right)} \end{bmatrix} \tag{A.15}$$

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 (A.9) to obtain:

$$\frac{\mathrm{d}p}{\mathrm{d}f} = \frac{\det(Z_p)}{\det(Z)} = \frac{(1-\beta)(rv+\delta)}{\Delta} > 0, \tag{A.16}$$

$$\frac{\mathrm{d}e}{\mathrm{d}f} = \frac{\det(Z_e)}{\det(Z)} = \frac{(1-v)(1-\beta)\delta}{\Delta e c''} > 0, \tag{A.17}$$

$$\frac{\mathrm{d}v}{\mathrm{d}f} = \frac{\det(Z_v)}{\det(Z)} = -\frac{(1-v)^2(1-\beta)\delta v}{\Delta e^2 c''} < 0 \tag{A.18}$$

where $\Delta = rv + \delta\big(1 - \beta(1 - v)\big)$. Because $\beta < 1$, the impact of footfall on rents is positive, $\mathrm{d}p/\mathrm{d}f > 0$. Furthermore, because the cost function is convex, we have $c'' > 0$, so that $\mathrm{d}e/\mathrm{d}f > 0$. This implies that advertising expenditure will increase when footfall is higher. Consequently, when advertising expenditure increases, the matching rate will also increase implying that $\mathrm{d}v/\mathrm{d}f < 0$ (see Eq. (A.8), which is confirmed by Eq. (A.18)). □

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

A2. Endogenous footfall

Until now, we assumed that footfall is exogenous. However, footfall likely depends negatively on the vacancy rate of shops in the shopping street, and is therefore endogenous. To take this feature into account, we allow footfall in the shopping street to fall with the vacancy rate of shops. More specifically, 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 occupied by retail firms. This assumption implies that there is a negative external effect of vacant shops, because a vacant shop reduces footfall. To investigate the effects of f and $\bar{f}$ 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 2. *When footfall is proportional to the occupancy rate of shops, (i)* $\mathrm{d}p/\mathrm{d}\bar{f} > \mathrm{d}p/\mathrm{d}f$ *and (ii)* $\mathrm{d}v/\mathrm{d}\bar{f} < \mathrm{d}v/\mathrm{d}f$.

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

$$\frac{\mathrm{d}v}{\mathrm{d}\bar{f}} = -\frac{\delta v(1-v)^3(1-\beta)}{\Delta e^2 c'' - (1-v)^2(1-\beta)\delta v\bar{f}}. \tag{A.19}$$

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

$$\frac{\mathrm{d}^2 v}{\mathrm{d}\bar{f}^2} = -\frac{(1-v)^5 v^2 (1-\beta)^2 \delta^2}{(e^2\Delta c'' - (1-v)^2(1-\beta)\delta v\bar{f})^2} < 0. \tag{A.20}$$

Using implicit differentiation, it should hold that:

$$\frac{\mathrm{d}p}{\mathrm{d}f} = \frac{\mathrm{d}p}{\mathrm{d}\bar{f}} \Big/ \frac{\mathrm{d}f}{\mathrm{d}\bar{f}} \quad \text{and} \quad \frac{\mathrm{d}v}{\mathrm{d}f} = \frac{\mathrm{d}v}{\mathrm{d}\bar{f}} \Big/ \frac{\mathrm{d}f}{\mathrm{d}\bar{f}}. \tag{A.21}$$

So if $\mathrm{d}f/\mathrm{d}\bar{f} > 1$, it holds that $\mathrm{d}p/\mathrm{d}\bar{f} > \mathrm{d}p/\mathrm{d}f$ and $\mathrm{d}v/\mathrm{d}\bar{f} < \mathrm{d}v/\mathrm{d}f$. Hence:

$$\frac{\mathrm{d}f}{\mathrm{d}\bar{f}} = -\frac{\mathrm{d}v}{\mathrm{d}\bar{f}}\bar{f} + (1 - v) > 1, \tag{A.22}$$

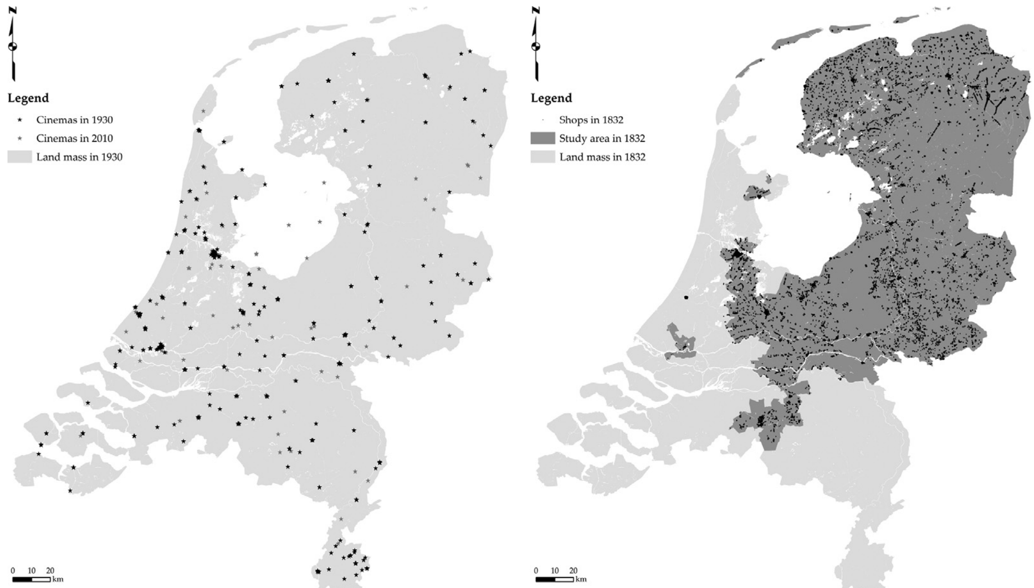
implying that $-\mathrm{d}v\mathrm{d}\bar{f} > \mathrm{d}\bar{f}$. Because $\mathrm{d}^2 v/\mathrm{d}\bar{f}^2 < 0$, this condition holds. □

Appendix B

B1. Historic data

In our empirical framework we use historic data going back to 1930. From *SpinLab* we gather information on the exact location of cinemas in 1930, as well as in 2010. It appears that in 1930 there were 315 cinemas, while in 2010 this was 180. In Fig. B.1(a) we show the spatial distribution of cinemas in 1930 and 2010. Unsurprisingly, cinemas in 1930 were mainly concentrated in cities. When replicating Fig. 1, but adding cinemas in 1930, we see that there are 17 cinemas located in the central shopping district, as shown in Fig. B.2, which are particularly concentrated in high-footfall parts of the shopping district. Also for other cities, historic cinemas seem to be particularly concentrated in high footfall areas of shopping streets. This suggests a strong autocorrelation of shopping streets of the past with current ones. On the other hand, the current locational preferences for cinemas seems to have changed somewhat: a decent share of the cinemas is now located in the suburbs close to highway ramps. Indeed, in Table B.1 we show that the number of cinemas in 2010 within 200 m of a shop is more than 50% lower than it was in 1930. On average, the number of cinemas within 200 m in 1930 is about 0.7, with a few locations having 6 cinemas in close vicinity.

We further use data from *HISGIS*. These data are based on a digitised and geocoded version of the first Dutch census of 1830. It is the oldest

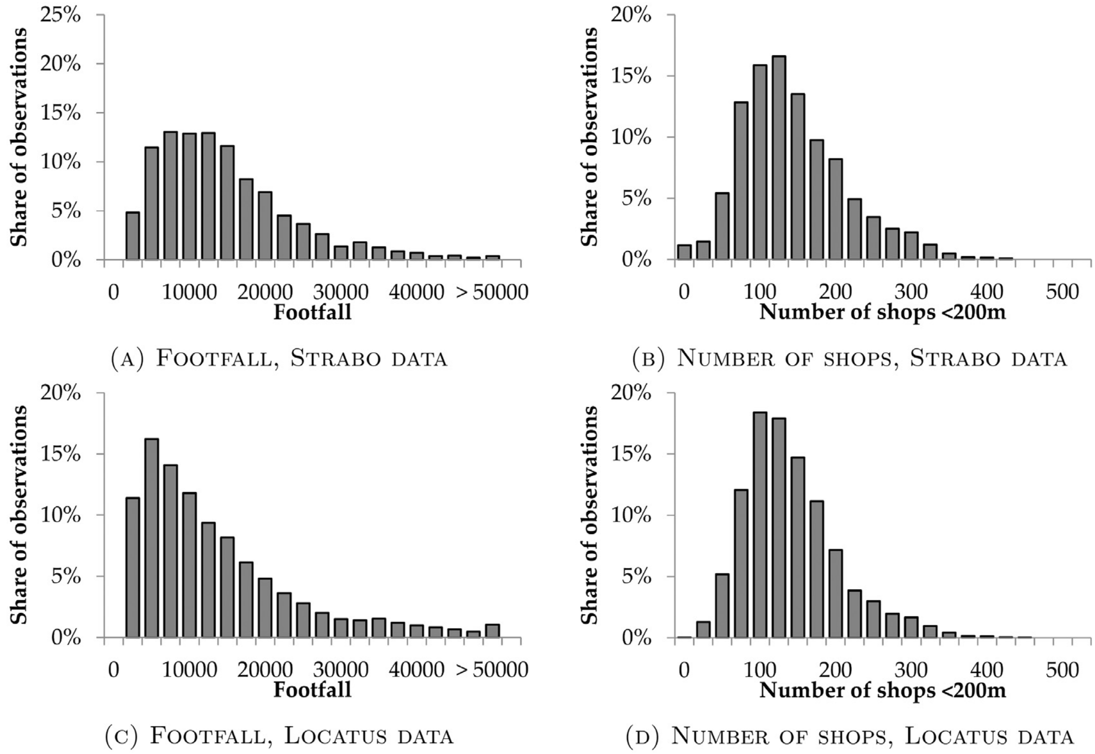


(A) Cinemas in 1930 and 2010 (B) Spatial distribution of shops in 1832

Fig. B.1. Maps of historic data.

Fig. B.2. Sample map for the Amsterdam city centre with historic variables.

nationwide registration system of property and land ownership, which was then used to determine land and property taxes. We have information on the footprint size of each building and for each parcel we have information on the owner and the owner's occupation. Based on the occupation and whether there was a building located on the parcel, we determine whether a building was used as a shop. More specifically, we have a list of a slightly more than 10 thousand occupations. We went through them one by one to determine whether the person is likely to sell something – so that his or her building can be qualified as a shop. Since the year 1832 was a time before industrialisation started in the Netherlands (and therefore many people were craftsmen), we classify about one third of the occupations as (related to) retail. In Fig. B.1(b) we show the study area and the general spatial distribution of shops in 1832. Unsurprisingly, they were located in built-up areas. Looking more closely at Amsterdam in Fig. B.2, we see that shops are particularly concentrated in what is Amsterdam's main shopping street: the Kalverstraat.



(A) Footfall, Strabo data
(B) Number of shops, Strabo data
(C) Footfall, Locatus data
(D) Number of shops, Locatus data

Fig. B.3. Histograms of main independent variables.

Table B.1
Descriptive statistics for historic variables.

Panel A: Strabo data	(1) mean	(2) sd	(3) min	(4) max
Number of cinemas in 1930, <200 m	0.680	0.923	0	6
Number of cinemas in 2010, <200 m	0.322	0.590	0	4
Number of shops in 1832, <200 m	173.4	156.0	0	595
Number of buildings in 1832, <200 m	388.6	316.4	0	1642
Cadastral income *(in guilders per hectare)*	4237	4701	10.88	176,755
Cadastral income is zero in 1832	0.202	0.402	0	1
Panel B: Locatus data	(1) mean	(2) sd	(3) min	(4) max
Number of cinemas in 1930, <200 m	0.701	0.981	0	6
Number of cinemas in 2010, <200 m	0.308	0.554	0	4
Number of shops in 1832, <200 m	153.9	160.6	0	654
Number of buildings in 1832, <200 m	256.1	242.9	0	1179
Cadastral income *(in guilders per hectare)*	4183	6523	2.258	520,298
Cadastral income is zero in 1832	0.256	0.436	0	1

Notes: For the *Strabo* data, the number of observations is 4378 for data from 1930 and 2496 for the data from 1832. For *Locatus* it is 410,544 for 1930 and 218,271 for 1832.

For each parcel we also have information on the so-called *Cadastral Income*, which is a proxy of the land value, as land and property taxes were based on this value. We report descriptive statistics for the variables in Table B.1. On average there are around 160 shops within 200 m. This number is slightly higher than the current number of shops in the vicinity (see Tables 1 and 2). This is likely because buildings were smaller in the past and also maybe because of our shop definition in 1832. The average of the Cadastral income per hectare is about 4 thousand Guilders. However, for some parcels, it is much higher. Table B.1 also shows that for about 25% of our observations, Cadastral income is missing, which is either due to missing information (*e.g.* for the province of Overijssel, we do not have information on the Cadastral income), or because there was no land at that time. Large parts of the Netherlands (such as Flevoland and areas close to Amsterdam) have been reclaimed from the sea in more recent years.

B2. Histograms

Fig. B.3 shows histograms of footfall and the number of shops within 200 m, respectively, for the *Strabo* and *Locatus* datasets. Both figures show that there are relative few observations with the lowest values of footfall and number of shops. Most of our observations are located in relatively dense areas where footfall is between 10,000 and 25,000, and the number of shops within 200 m (from a shop) is between 100 and 250 shops (see Fig. B.3(a) and (b)). The distributions of the *Locatus* data are similar, although the distribution of footfall is more skewed to the right (see Fig. B.3(c)).

Appendix C

C1. Estimating heterogeneous parameters

We aim to estimate:

$$\log p_{ijt} = \alpha_i \log s_{ijt} + \beta_i x_{ijt} + \varphi_j + \psi_{i\in k} + \theta_t + \epsilon_{ijt}. \tag{C.1}$$

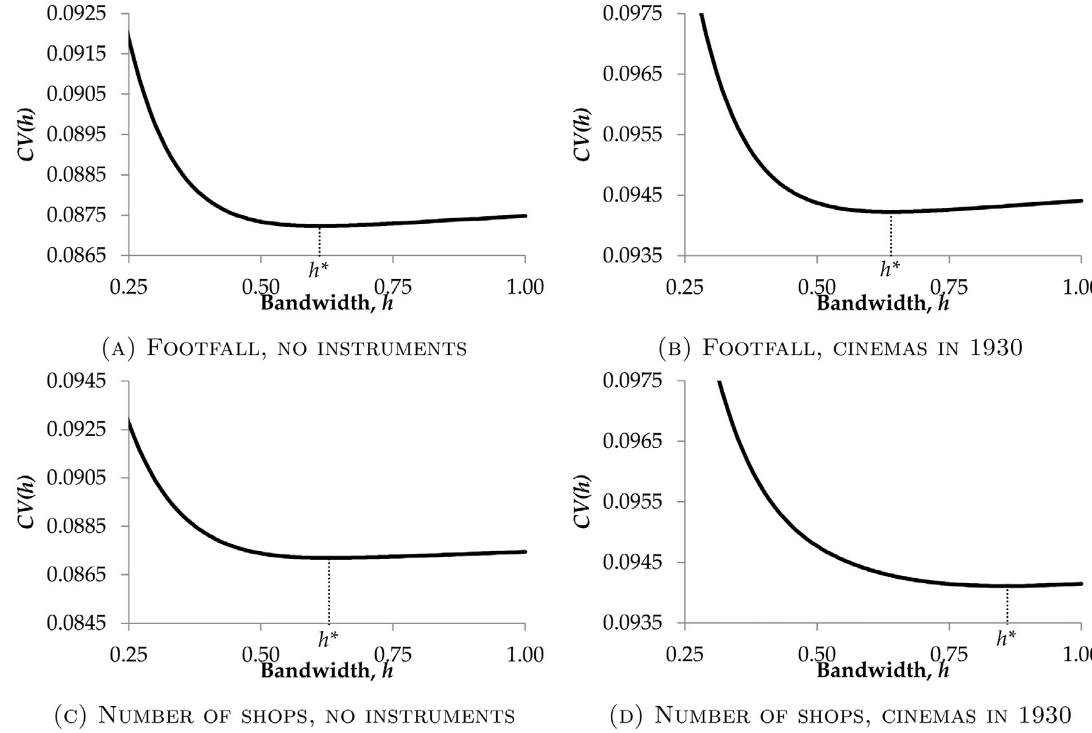
where α_i and β_i are retail firm specific parameters.

Following Bishop and Timmins (2018) we can condition out the fixed effects by taking a Taylor series expansion around observation *i*. Let then $\log \tilde{p}_{ijt}$, $\log \tilde{s}_{ijt}$ and $\tilde{x}_{ijt}$ denote demeaned values. We then estimate:

$$(\hat{\alpha}, \hat{\beta}_i) = \arg\min_{\alpha_i, \beta_i} \sum_{n=1}^{N} K\left(\frac{z_{ik} - z_k}{h}\right) \times (\log \tilde{p}_{ijt} - \alpha_i \log \tilde{s}_{ijt} - \beta_i \tilde{x}_{ijt})^2. \tag{C.2}$$

where z_{ik} are retail firm characteristics, where $k = 1, \dots, \mathscr{K}$ and $K(\cdot)$ is a kernel function:

$$K\left(\frac{z_{ik} - z_k}{h}\right) = \prod_{k=1}^{\mathscr{K}} w_{ik}(z_{ik}, h), \tag{C.3}$$



(A) Footfall, no instruments

(B) Footfall, cinemas in 1930

(C) Number of shops, no instruments

(D) Number of shops, cinemas in 1930

Fig. C.1. Cross-validation scores.

Table C.1
Heterogeneity: first-step results.

Panel A: No instruments	$\varepsilon_{I,f}$			$\varepsilon_{I,N}$		
	(1) 25th perc.	(2) 50th perc.	(3) 75th perc.	(4) 25th perc.	(5) 50th perc.	(6) 75th perc.
$\hat{\varepsilon}$	0.3087***	0.3148***	0.3247***	0.1896***	0.1986***	0.2087***
	(0.0194)	(0.0202)	(0.0218)	(0.0366)	(0.0368)	(0.0373)
Number of observations	4378			4378		
Cost parameter, κ	0.426			0.426		
Bandwidth, h^*	0.611			0.64		
Panel B: Cinemas in 1930	$\varepsilon_{I,f}$			$\varepsilon_{I,N}$		
	(1) 25th perc.	(2) 50th perc.	(3) 75th perc.	(4) 25th perc.	(5) 50th perc.	(6) 75th perc.
$\hat{\varepsilon}$	0.4767***	0.5129***	0.56***	0.5206***	0.5268***	0.5364***
	(0.1002)	(0.1023)	(0.106)	(0.1372)	(0.1388)	(0.1416)
Number of observations	4,378			4,378		
Cost parameter, κ	0.426			0.426		
Bandwidth, h^*	0.629			0.86		

Notes: In all regressions we include shop and location characteristics, as well as shopping street fixed effects, retail sector × branch fixed effects and year fixed effects. The estimated elasticities are based on estimates of the corresponding semi-elasticity of vacancies with respect to shopping externalities. We recover $\varepsilon_{f,N} = \varepsilon_{I,N}/\varepsilon_{I,f}$. In Panels B and C we take a control function approach, where we use respectively cinemas in 1930 and shops in 1832 as instruments. Bootstrapped standard errors (100 replication) are clustered at the postcode and in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

where w_{ik} are kernel weights. We solely use categorical firm characteristics. That is, we use identifier variables for each retail firm, identified by its chain (if any), the 2-digit retail industry, 5-digit retail sector and the 8-digit retail branch, as well as two indicators whether the shop is small (lowest 25th percentile), medium or large (highest 25th percentile). Following Racine and Li (2004) and Racine et al. (2006), we define:

$$w_{ik} = \begin{cases} 1 & z_{ik} = z_k \\ h & z_{ik} \neq z_k \end{cases} \tag{C.4}$$

The above specification of the kernel ensures that observations that are part of the same chain (and therefore share the same branch code) and occupy shops of similar sizes have a weight of 1. More generally, retail firms that are more similar in observable firm characteristics (*e.g.* belong to the same industry) will have a higher kernel weight in the regression of i and therefore have more similar parameters $\hat{\alpha}_i$ and $\hat{\beta}_i$

An important parameter in the kernel function is the bandwidth h, which governs the degree of smoothing. Note that h is between 0 and 1, with $h = 0$ implying that we include interaction terms of $\tilde{s}_{ijt}$ and $\tilde{x}_{ijt}$ with each unique combination of z_{ik}. When $h = 1$, we obtain identical parameters for each firm, which are the same as the OLS results.

The question is then what is the right choice for h. Following the literature, we use a leave-out cross-validation procedure (see *e.g.*

Silverman, 1986). This implies:

$$CV(\hat{h}) = \underset{CV(h)}{\arg\min} \frac{1}{N} \sum_{n=1}^{N} (\log \tilde{p}_{ijt} - \log \tilde{p}_{-ijt}(h))^2 \tag{C.5}$$

Because errors are likely correlated within postcodes, we do not leave one observation out at a time, but all observations within a postcode (see Opsomer et al., 2001).

We would finally like to make a note on how to use instrumental variables in this setting. When using semi-parametric models as above, standard two-stage least squares estimates yield inconsistent estimates (Blundell and Powell, 2003). As in Koster et al. (2014), we therefore rely on a control function approach, where the first-stage residual is inserted in the second stage as a control function. As a first stage, we then have:

$$\log s_{ijt} = \tilde{\delta}_i \log z_{ij} + \tilde{\beta}_i x_{ijt} + \tilde{\gamma}_i h_{ij} + \tilde{\varphi}_j + \tilde{\psi}_{i\in j} + \tilde{\theta}_t + \xi_{ijt}, \tag{C.6}$$

where ξ_{ijt} is the first-stage residual. The second stage yields:

$$\log p_{ijt} = \alpha_i \log s_{ijt} + \beta_i x_{ijt} + \gamma_i h_{ij} + \rho_i \hat{\xi}_{ijt} + \varphi_j + \psi_{i\in k} + \theta_t + \epsilon_{ijt}. \tag{C.7}$$

where $\rho_i \hat{\xi}_{ijt}$ captures the 'control function'.

C2. Determining the bandwidth

In Fig. C.1 we show the cross-validation scores for 4 different specifications. We show that for most specifications, the optimal bandwidth is around 0.65. Only when looking at the impact of number of shops on rents, while instrumenting with the number of cinemas in 1930, we find a somewhat higher bandwidth, equal to 0.86 (see Fig. C.1(d)).

We also make sure that the optimal bandwidths do not change considerably once we pursue a leave-one-out cross-validation approach rather than leaving one postcode out at a time. It appears that the optimal bandwidths are then only maximally 5% smaller.

C3. Other results of the first step

Here we report additional results of the first step of the estimation procedure. Hence, we calculate the elasticity of rental income with respect to footfall, $\epsilon_{I,f}$ and the elasticity of rental income with respect to the number of shops $\epsilon_{I,N}$. To be able to do this we use $\partial v_{ij}/\partial \log s_{ij}$ from Tables 6 and 7. Note that these estimates are assumed to be the same for all firms. We further assume that $\kappa = 0.426$, in line with Section 5.3.

We report results in Table C.1, where we report the relevant elasticities for the 25th percentile, the median and the 25th percentile. We calculate cluster-bootstrapped standard errors. It is again confirmed that there is limited heterogeneity. Moreover, the results are often quite precise, in particular when we do not instrument for footfall and the number of shops.

Appendix D

D1. First-stage results

We report first-stage results when using the *Strabo* dataset in Table D.1. Columns (1) and (2) use the number of cinemas in 1930 to explain the current footfall. The instrument is sufficiently strong with an *F*-statistic exceeding 40. The coefficients imply that a standard deviation increase in the number of cinemas in 1930 increases footfall by about 12%. This effect is essentially the same regardless of controlling for the number of cinemas in 2010.

In columns (3)–(5) of Table D.1 we use instruments based on data from 1832. It appears that the number of shops in 1832 is a strong determinant of current footfall: a standard deviation increase in the number of shops in 1832 raises current footfall by about 35%. The coefficient is insensitive to the inclusion of control variables, such as Cadastral Income (column (4)) and the number of buildings in 1832 (column (5)).

Columns (6)–(10) in Table D.1 repeat the same set of specifications, but now we use the number of shops as the endogenous variable. The results suggest that the instruments have very similar impacts on the number of shops as on footfall. We note that this does not always fold for control variables. For example, while the number of cinemas in 2010 has a negative impact on footfall, it does not have a statistically significant impact on the number of shops (see columns (2) and (7)). Furthermore, Cadastral Income seems to have a positive impact on the number of shops, while the impact on footfall is not significantly different from zero (see columns (4) and (9)).

For completeness we also report the first-stage results when relying on the *Locatus* data in Table D.2. The number of observations is almost 100 times higher, but the first-stage results are qualitatively and quantitatively very similar to the ones reported in Table D.1

D2. Omitted variable bias revisited

In the empirical literature, non-experimental papers often investigate coefficient movements of the variables of interest after the inclusion of additional control variables to obtain information on whether omitted variable bias is important. Oster (2019) argues that solely using coefficient movements is not sufficient. Instead, she argues that the potential bias depends on the statistical relevance of the added control variables in explaining the dependent variable, *together* with any coefficient movements. Here, we investigate the effect of two types of additional control variables, shop and location characteristics, whereas we keep a range of fixed effects (year fixed effects, shopping district fixed effects, shopping street fixed effects, retail sector X chain fixed effects) as basic controls.

Oster (2019) derives an estimator to correct estimates for omitted variable bias. The idea is that the increase in the R^2 when adding additional control variables is informative on the magnitude of the bias when these additional control variables are correlated to unobservables. For this GMM-estimator, there are two key input parameters that have to be determined. First, there is the maximum R^2 from a hypothetical regression of rents on footfall or number of shops and all theoretically possible controls, which we denote as $\bar{R}^2$. We set $\bar{R}^2$ to 1, which will amplify any potential bias, as it is plausible that $\bar{R}^2$ will be (much) lower than 1 (see Oster, 2019). Second, a parameter must be chosen that determines the relative degree of selection on observed and unobserved variables, which we denote by ω. Although this parameter is fundamentally unknown, Altonji et al. (2005) and Oster (2019) show that $\omega = 1$ is a reasonable (upper-bound) value. It appears that:

$$\alpha^* \approx \hat{\alpha} - \omega(\mathring{\alpha} - \hat{\alpha}) \frac{\bar{R}^2 - \hat{R}^2}{\hat{R}^2 - \mathring{R}^2}, \tag{D.1}$$

where α^* is the bias-corrected estimate for the effect of footfall on rents. $\hat{\alpha}$ is the parameter estimate obtained from a regression which includes all controls, so the additional controls (i.e. shop and location characteristics) and basic controls (different types of fixed effects), and $\hat{R}^2$ is the corresponding R^2. $\mathring{\alpha}$ is the parameter estimate obtained from a regression with the basic controls and $\mathring{R}^2$ is the corresponding R^2. Hence, this equation provides a straightforward way to evaluate robustness of the results. We report bootstrapped bias-corrected estimates in Table D.3, which replicates the baseline table for rents (see Table 3).

In columns (1)–(3) we replicate the results for footfall. Compared to the baseline estimates, the coefficients are very similar and differ maximally 2.6 percentage points from the baseline estimate. For the preferred specification in column (3) the bias-corrected estimate is only 0.33 percentage points (1.2%) higher. For number of shops the difference between the baseline estimates and the bias-corrected estimates are still small, albeit somewhat larger. For column (4), the difference is 3.6 percentage points, while for the preferred specification the difference is 1.56 percentage points. Furthermore, when looking at the standard errors, the bias-corrected estimates are not statistically significantly different from the OLS specifications. We therefore conclude that omitted

Table D.1
First-stage results for Strabo data.

	Dependent variable: log of footfall					*Dependent variable: log of the number of shops <200 m*				
	Cinemas in 1910		*Number of shops in 1832*			*Cinemas in 1910*		*Number of shops in 1832*		
	(1) OLS	(2) OLS	(3) OLS	(4) OLS	(5) OLS	(6) OLS	(7) OLS	(8) OLS	(9) OLS	(10) OLS
Number of cinemas in 1930, <200 m *(std)*	0.1175*** (0.0180)	0.1295*** (0.0186)				0.1237*** (0.0177)	0.1251*** (0.0183)			
Number of cinemas in 2010, <200 m *(std)*		−0.0509*** (0.0169)					−0.0059 (0.0143)			
Number of shops in 1832, <200 m *(std)*			0.3533*** (0.0637)	0.3601*** (0.0630)	0.3462*** (0.0817)			0.4186*** (0.0644)	0.4594*** (0.0583)	0.3658*** (0.0982)
Cadastral income *(log)*				0.0448 (0.0380)	0.0444 (0.0382)				0.1338*** (0.0503)	0.1308*** (0.0505)
Cadastral income is zero in 1832				0.8952** (0.4096)	0.8927** (0.4103)				0.8723* (0.4484)	0.8551* (0.4507)
Number of buildings in 1832, <200 m *(std)*					0.0168 (0.0709)					0.1132 (0.1165)
Shop characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Location characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Shopping street fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Retail sector × chain fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	4378	4378	4378	2496	2496	4378	4378	4378	2496	2496
R^2	0.780	0.781	0.780	0.776	0.776	0.838	0.838	0.839	0.861	0.861

Notes: Footfall is measured as the number of shoppers per day. Shop characteristics include property size, whether the building is new or renovated and construction year dummies, which are categorised as follows: <1832, 1832–1930, 1931–1950, 1951–1960, 1961–1970, 1971–1980, 1981–1990, 1991–2000, >2000. Location characteristics include whether the property is in a historic district, and the number of geocoded pictures <200 m, religious buildings <200 m, bus stops <200 m, public buildings <200 m, schools, and railway stations <200 m. Robust standard errors are clustered at the postcode and in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

Table D.2
First-stage results for Locatus data.

	Dependent variable: log of footfall					*Dependent variable: log of the number of shops <200 m*				
	Cinemas in 1910		*Number of shops in 1832*			*Cinemas in 1910*		*Number of shops in 1832*		
	(1) OLS	(2) OLS	(3) OLS	(4) OLS	(5) OLS	(6) OLS	(7) OLS	(8) OLS	(9) OLS	(10) OLS
Number of cinemas in 1930, <200 m *(std)*	0.0965*** (0.0113)	0.1028*** (0.0117)				0.1438*** (0.0084)	0.1480*** (0.0086)			
Number of cinemas in 2010, <200 m *(std)*		−0.0258** (0.0109)					−0.0175*** (0.0067)			
Number of shops in 1832, <200 m *(std)*			0.3841*** (0.0360)	0.3926*** (0.0356)	0.2904*** (0.0522)			0.3938*** (0.0233)	0.4361*** (0.0232)	0.4361*** (0.0232)
Cadastral income *(log)*				−0.0267 (0.0177)	−0.0273 (0.0176)				0.0025 (0.0069)	0.0025 (0.0069)
Cadastral income is zero in 1832				−0.4569* (0.2452)	−0.4617* (0.2456)				−0.1292 (0.0914)	−0.1292 (0.0914)
Number of buildings in 1832, <200 m *(std)*					0.1199*** (0.0462)					
Shop characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Location characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Shopping street fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Retail sector × chain fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	410,544	410,544	410,544	218,271	218,271	410,544	410,544	410,544	218,271	218,271
R^2	0.689	0.690	0.691	0.696	0.696	0.789	0.790	0.785	0.834	0.834

Notes: Footfall is measured as the number of shoppers per day. Shop characteristics include building surface and construction year dummies, which are categorised as follows: <1832, 1832–1930, 1931–1950, 1951–1960, 1961–1970, 1971–1980, 1981–1990, 1991–2000, >2000. Location characteristics include whether the property is in a historic district, and the number of geocoded pictures <200 m, religious buildings <200 m, bus stops <200 m, public buildings <200 m, schools, and railway stations <200 m. Robust standard errors are clustered at the postcode and in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

variables are unlikely to play an important role, which is in line with many other sensitivity checks.

D3. Sensitivity for retail rents

In this Appendix section we illustrate the robustness of the results for the *Strabo* data. We first estimate several regressions that should contribute to the belief that our identification strategy is valid. Table D.4 reports the results.

In Panel A we focus on footfall and in Panel B on the number of shops. We first consider an alternative identification strategy where we compare shops near shopping street intersections. Hence, rather than identifying the effects *within* shopping streets, we now compare differences in footfall and number of shops *between* shopping streets that in-

Table D.3
Bias-corrected results for retail rents.

	Footfall			*Number of shops*		
	(1) GMM	(2) GMM	(3) GMM	(4) GMM	(5) GMM	(6) GMM
Footfall *(log)*	0.4000***	0.3108***	0.2642***			
	(0.0183)	(0.0277)	(0.0274)			
Number of shops, <200 m *(log)*				0.2150***	0.2000***	0.1669***
				(0.0307)	(0.0607)	(0.0584)
Additional controls						
Shop characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Location characteristics	No	Yes	Yes	No	Yes	Yes
Basic controls						
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Shopping district fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Shopping street fixed effects	No	Yes	Yes	No	Yes	Yes
Retail sector × chain fixed effects	No	No	Yes	No	No	Yes
$\bar{R}^2$	1.000	1.000	1.000	1.000	1.000	1.000
ω	1.000	1.000	1.000	1.000	1.000	1.000
Number of observations	4378	4378	4378	4378	4378	4378

Notes: Footfall is measured as the number of shoppers per day. We include the fixed effects (at the year, shopping district, shopping street and/or retail sector × chain) as controls that are assumed to be unrelated to the set of proportionally related unobservables. The construction year dummies are categorised as follows: <1832, 1832–1930, 1931–1950, 1951–1960, 1961–1970, 1971–1980, 1981–1990, 1991–2000, >2000. Location characteristics include whether the property is in a historic district, and the number of geocoded pictures <200 m, religious buildings <200 m, bus stops <200 m, public buildings <200 m, schools, and railway stations <200 m. Bootstrapped standard errors (250 replications) are clustered at the postcode and in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table D.4
Retail rents: Identification revisited.

(Dependent variable: log of rent per m^2)								
	Intersection fixed effects			*Real estate agent fixed effects*			*Sales prices*	
		Cinemas in 1930	*Shops in 1832*		*Cinemas in 1930*	*Shops in 1832*		
Panel A: Footfall	(1) OLS	(2) 2SLS	(3) 2SLS	(4) OLS	(5) 2SLS	(6) 2SLS	(7) OLS	(8) OLS
Footfall *(log)*	0.2146***	0.3158**	0.4324	0.2069***	0.3825***	0.3136	0.1991*	0.1591
	(0.0218)	(0.1541)	(0.3481)	(0.0262)	(0.0994)	(0.1934)	(0.1063)	(0.1800)
Shop characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Location characteristics	Yes	Yes	Yes	Yes	Yes	Yes	No	No
Intersection fixed effects	Yes	Yes	Yes	No	No	No	No	No
Shopping street fixed effects	No	No	No	Yes	Yes	Yes	No	Yes
Retail sector × chain fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	No	No
Real estate agent fixed effects	No	No	No	Yes	Yes	Yes	No	No
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	4136	4136	2340	4378	4378	2496	110	110
R^2	0.836			0.722			0.414	0.997
Kleibergen Paap F-statistic		23.40	5.418		43.38	14		
Endogeneity test		0.624	0.447		4.296	0.169		
$\xi^2(2)$ p-value		0.430	0.504		0.0382	0.681		
Panel B: Number of shops	(1) OLS	(2) 2SLS	(3) 2SLS	(4) OLS	(5) 2SLS	(6) 2SLS	(7) OLS	(8) OLS
Number of shops, <200 m *(log)*	0.1662***	0.2761*	0.6279	0.1330***	0.4613***	0.2901	0.0753	0.1368
	(0.0325)	(0.1435)	(0.5772)	(0.0406)	(0.1368)	(0.2163)	(0.0893)	(0.1833)
Shop characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Location characteristics	Yes	Yes	Yes	Yes	Yes	Yes	No	No
Intersection fixed effects	Yes	Yes	Yes	No	No	No	No	No
Shopping street fixed effects	No	No	No	Yes	Yes	Yes	No	Yes
Retail sector × chain fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	No	No
Real estate agent fixed effects	No	No	No	Yes	Yes	Yes	No	No
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	4136	4136	2340	4378	4378	2496	110	110
R^2	0.830			0.716			0.391	0.996
Kleibergen Paap F-statistic		34.41	5.698		35.79	10.70		
Endogeneity test		0.886	1.281		9.882	0.864		
$\xi^2(2)$ p-value		0.346	0.258		0.00167	0.353		

Notes: Footfall is measured as the number of shoppers per day. Shop characteristics include property size, whether the building is new or renovated and construction year dummies, which are categorised as follows: <1832, 1832–1930, 1931–1950, 1951–1960, 1961–1970, 1971–1980, 1981–1990, 1991–2000, >2000. Location characteristics include whether the property is in a historic district, and the number of geocoded pictures <200 m, religious buildings <200 m, bus stops <200 m, public buildings <200 m, schools, and railway stations <200 m. Robust standard errors are clustered at the postcode and in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

Table D.5
Retail rents and footfall: Non-linear effects and internalisation.

(Dependent variable: log of rent per m²)	*Non-linear effects*			*Multi-property owners*			*Private investors*			*Retail associations*		
		Cinemas in 1930	*Shops in 1832*		*Cinemas in 1930*	*Shops in 1832*		*Cinemas in 1930*	*Shops2 in 1832*		*Cinemas in 1930*	*Shops in 1832*
	(1) OLS	(2) 2SLS	(3) 2SLS	(4) OLS	(5) 2SLS	(6) 2SLS	(7) OLS	(8) 2SLS	(9) 2SLS	(10) OLS	(11) 2SLS	(12) 2SLS
Footfall *(log)* − $\overline{\text{Footfall}(log)}$	0.2952***	0.4455***	0.4123**									
	(0.0201)	(0.1000)	(0.1664)									
(Footfall *(log)* − $\overline{\text{Footfall}(log)}$)^2	0.0582***	0.2127	0.2297									
	(0.0106)	(0.4135)	(0.3332)									
Footfall *(log)*				0.2430***	0.2132	0.1994	0.2534***	0.2734	0.5454*	0.2626***	0.4279***	0.4562*
				(0.0391)	(0.2848)	(0.2540)	(0.0284)	(0.2112)	(0.2859)	(0.0187)	(0.1039)	(0.2336)
Footfall *(log)* × Multi-property owner				−0.0011	0.3170	−0.0539						
				(0.0615)	(0.3432)	(0.3127)						
Multi-property owner				0.0249	−3.0065	0.5290						
				(0.5862)	(3.2847)	(2.9892)						
Footfall *(log)* × Private investor							0.0167	0.2363	−0.0964			
							(0.0280)	(0.1728)	(0.1115)			
Private investor							−0.1487	−2.1945	0.9297			
							(0.2623)	(1.6172)	(1.0475)			
Footfall *(log)* × in retail association area										−0.0201	−0.2104	0.1951
										(0.0519)	(0.6367)	(0.4743)
In retail association area										0.1478	1.8835	−1.7623
										(0.4934)	(6.0040)	(4.4137)
Shop and location characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Shopping street fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Retail sector × chain fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	4378	4378	2496	1614	1614	900	3565	3565	2047	4378	4378	2496
R^2	0.823			0.891			0.825			0.821		
Kleibergen Paap *F*-statistic		0.684	1.308		3.604	4.778		5.605	5.980		1.127	6.056
Endogeneity test		3.555	0.576		2.495	0.302		11.11	1.453		4.614	2.598
$\xi^2(2)$ *p*-value		0.169	0.750		0.287	0.860		0.00387	0.484		0.0996	0.273

Notes: Footfall is measured as the number of shoppers per day. The construction year dummies are categorised as follows: <1832, 1832–1930, 1931–1950, 1951–1960, 1961–1970, 1971–1980, 1981–1990, 1991–2000, >2000. Location characteristics include whether the property is in a historic district, and the number of geocoded pictures <200 m, religious buildings <200 m, bus stops <200 m, public buildings <200 m, schools, and railway stations <200 m. Robust standard errors are clustered at the postcode and in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

tersect.[44] We then only keep observations within 250 m of an intersection and include unique fixed effects for each shopping street intersection. We show that the results are very much comparable to the baseline specifications. By using spatial variation in footfall between intersecting streets, we control for unobserved locational endowments that attract both shoppers and retail firms (*e.g.* free parking etc.). The coefficients when using cinemas in 1930 as an instrument (column (2)) seem somewhat lower, but they appear not to be statistically significantly different from the baseline estimates. The coefficients when using the number of shops in 1832 are not statistically significant at conventional levels. This is mainly because with so many local fixed effects, the instrument is not sufficiently strong.

One may argue that different real estate agents have different bargaining power or sales strategies, which may lead to different observed rents and/or unreported incentives. In columns (4)–(6), we make sure that when including real estate agent fixed effects the results do not materially change. Again, it is shown that the coefficients using the number of shops in 1832 become imprecise when including this additional set of fixed effects. However, the point estimates still have a similar magnitude.

To avoid any reverse causality between rents or vacancy rates and shopping externalities, we may also consider to use sales price directly. The main reason why we do not prefer this is that only a very small share of transactions are sales transactions. Hence, it appears that for our sample period and for shopping streets we are left with only 110 observations. This implies that we cannot include as many fixed effects and controls as in the baseline specifications. Also, the instruments appear not to have sufficient power to lead to meaningful results with this low number of observations. Keeping these caveats in mind, in Panel A, column (7) we only control for shop characteristics and year fixed effects. We then find a positive effect of footfall on sales prices, which is surprisingly close to the baseline estimate for rental income. When we control for shopping street fixed effects in Panel A, column (8), the point estimate remains similar but the coefficient becomes very imprecise. This is confirmed by regressions of the sales price on the number of shops. The point estimates are very comparable to the baseline estimates, but are too imprecise to draw any strong conclusions.

In Tables D.5 and D.6 we investigate whether non-linear effects are relevant and, importantly, whether there is any evidence that shop owners internalise shopping externalities.

In columns (1)–(3) of Table D.5 we show the results of the main rent specifications using the demeaned log footfall and its square, instead of the annual log footfall. In column (1) we estimate the preferred OLS specification. We find evidence that the elasticity of rents with respect to footfall is increasing in footfall. However, when instrumenting for footfall, this coefficient becomes highly statistically insignificant (see columns (2) and (3) in Panel A).

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 shop ownership, in particular in shopping streets. As an empirical test for this argument, we use the

[44] 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 low-footfall street.

Table D.6
Retail rents and number of shops: Non-linear effects and internalisation.

(Dependent variable: log of rent per m²)	*Non-linear effects*			*Multi-property owners*			*Private investors*			*Shop associations*		
		Cinemas in 1930	*Shops in 1832*		*Cinemas in 1930*	*Shops in 1832*		*Cinemas in 1930*	*Shops2 in 1832*		*Cinemas in 1930*	*Shops in 1832*
	(1) OLS	(2) 2SLS	(3) 2SLS	(4) OLS	(5) 2SLS	(6) 2SLS	(7) OLS	(8) 2SLS	(9) 2SLS	(10) OLS	(11) 2SLS	(12) 2SLS
Number of shops, <200 m *(log)* − $\overline{\text{Number of shops}, <200\ m(log)}$	0.3152***	0.4927*	0.4818*									
	(0.0333)	(0.2774)	(0.2696)									
(Number of shops, <200 m *(log)* − $\overline{\text{Number of shops}, <200\ m(log)}$)^2	0.0620***	−0.3411	0.1304									
	(0.0102)	(1.2955)	(0.1032)									
Number of shops, <200 m *(log)*				0.1473***	0.3839	0.1719	0.1132***	0.3111**	0.4388	0.1464***	0.4796***	0.5616
				(0.0533)	(0.3149)	(0.2221)	(0.0426)	(0.1551)	(0.3084)	(0.0329)	(0.1292)	(0.3626)
Number of shops, <200 m *(log)* × Multi-property owner				0.0124	0.0439	−0.0440						
				(0.0492)	(0.1678)	(0.1711)						
Multi-property owner				−0.0460	−0.1860	0.2129						
				(0.2314)	(0.7926)	(0.7877)						
Number of shops, <200 m *(log)* × Private investor							0.0685*	0.2558**	−0.0546			
							(0.0366)	(0.1152)	(0.1318)			
Private investor							−0.3277*	−1.2188**	0.2660			
							(0.1765)	(0.5473)	(0.6224)			
Number of shops, <200 m *(log)* × In shop association area										0.1164	−0.2975	0.1251
										(0.0751)	(0.1950)	(0.3570)
In shop association area										−0.5446	1.2871	−1.0005
										(0.3517)	(0.8902)	(1.4824)
Shop and location characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Shopping street fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Retail sector × chain fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	4378	4378	2496	1614	1614	900	3565	3565	2047	4378	4378	2496
R^2	0.812			0.885			0.812			0.807		
Kleibergen Paap *F*-statistic		0.108	2.948		2.573	12.38		16.34	8.804		21.75	8.742
Endogeneity test		2.914	0.505		3.020	0.434		14.20	6.742		13.31	3.329
$\xi^2(2)$ *p*-value		0.233	0.777		0.221	0.805		0.000826	0.0344		0.00129	0.189

Notes: Footfall is measured as the number of shoppers per day. The construction year dummies are categorised as follows: <1832, 1832–1930, 1931–1950, 1951–1960, 1961–1970, 1971–1980, 1981–1990, 1991–2000, >2000. Location characteristics include whether the property is in a historic district, and the number of geocoded pictures <200 m, religious buildings <200 m, bus stops <200 m, public buildings <200 m, schools, and railway stations <200 m. Robust standard errors are clustered at the postcode and in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

Table D.7
Retail rants and number of shops: Different thresholds.

	Number of shops <100m			*Number of shops <250m*		
	Baseline specification	*Cinemas in 1930 <100m*	*Shops in 1832 <100m*	*Baseline specification*	*Cinemas in 1930 <250m*	*Shops in 1832 <250m*
	(1) OLS	(2) 2SLS	(3) 2SLS	(4) OLS	(5) 2SLS	(6) 2SLS
Number of shops, <100m *(log)*	0.1393***	0.5644***	0.3177			
	(0.0198)	(0.1891)	(0.2258)			
Number of shops, <250m *(log)*				0.1384***	0.5057***	0.4010
				(0.0353)	(0.1329)	(0.3044)
Shop characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Location characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Shopping street fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Retail sector × chain fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Real estate agent fixed effects	No	No	No	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	4378	4109	2342	4378	4109	2342
R^2	0.809			0.806		
Kleibergen Paap *F*-statistic		16.16	21.01		38.33	9.559
Endogeneity test		8.427	0.876		12.15	1.335

Notes: Shop characteristics include property size, whether the building is new or renovated and construction year dummies, which are categorised as follows: <1832, 1832–1930, 1931–1950, 1951–1960, 1961–1970, 1971–1980, 1981–1990, 1991–2000, >2000. Location characteristics include whether the property is in a historic district, and the number of geocoded pictures <200 m, religious buildings <200 m, bus stops <200 m, public buildings <200 m, schools, and railway stations <200 m. Robust standard errors are clustered at the postcode and in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

Table D.8
Vacancies: Identification revisited.

(Dependent variable: shop is vacant)	*Intersection fixed effects*			*Booms and busts*			*Probit models*		
		Cinemas in 1930	*Shops in 1832*		*Cinemas in 1930*	*Shops in 1832*		*Cinemas in 1930*	*Shops in 1832*
Panel A: Footfall	(1) OLS	(2) 2SLS	(3) 2SLS	(4) OLS	(5) 2SLS	(6) 2SLS	(7) Probit	(8) IVProbit	(9) IVProbit
Footfall *(log)*	−0.0304***	−0.0616***	−0.0500*	−0.0276***	−0.0575***	0.0012	−0.0313***	−0.0702***	−0.0214
	(0.0016)	(0.0176)	(0.0258)	(0.0015)	(0.0098)	(0.0155)	(0.00116)	(0.0118)	(0.0155)
Footfall *(log)* × bust				−0.0124***	−0.0123**	−0.0296***			
				(0.0013)	(0.0057)	(0.0063)			
Shop and location characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Intersection fixed effects	Yes	Yes	Yes	No	No	No	No	No	No
Shopping street fixed effects	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	394,389	394,389	207,242	410,544	410,544	218,271	405,947	405,947	215,036
R^2	0.0448			0.0448					
Log-likelihood							−86,041	−434,705	−229,460
Kleibergen Paap *F*-statistic		37.37	9.777		42.33	22.20			
Endogeneity test		3.423	0.743		12.07	8.810			
$\xi^2(2)$ *p*-value		0.0643	0.389		0.00239	0.0122			
Panel B: Number of shops	(1) OLS	(2) 2SLS	(3) 2SLS	(4) OLS	(5) 2SLS	(6) 2SLS	(7) Probit	(8) IVProbit	(9) IVProbit
Number of shops, <200 m *(log)*	−0.0492***	−0.0559***	−0.0447**	−0.0438***	−0.0443***	0.0022	−0.0439***	−0.0563***	−0.0199
	(0.0037)	(0.0158)	(0.0228)	(0.0031)	(0.0080)	(0.0130)	(0.00254)	(0.00848)	(0.0154)
Number of shops, <200 m *(log)* × bust				−0.0121***	−0.0177***	−0.0299***			
				(0.0022)	(0.0052)	(0.0054)			
Shop and location characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Intersection fixed effects	Yes	Yes	Yes	No	No	No	No	No	No
Shopping street fixed effects	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	394,389	394,389	207,242	410,544	410,544	218,271	405,947	405,947	215,036
R^2	0.0409			0.0406					
Log-likelihood							−86,767	−120,178	−44,143
Kleibergen Paap *F*-statistic		233.9	75.33		142.7	48.83			
Endogeneity test		0.194	0.188		1.462	29.48			
$\xi^2(2)$ *p*-value		0.660	0.665		0.481	0.0000			

Notes: Footfall is measured as the number of shoppers per day. Shop characteristics include property size, whether the building is new or renovated and construction year dummies, which are categorised as follows: <1832, 1832–1930, 1931–1950, 1951–1960, 1961–1970, 1971–1980, 1981–1990, 1991–2000, >2000. Location characteristics include whether the property is in a historic district, and the number of geocoded pictures <200 m, religious buildings <200 m, bus stops <200 m, public buildings <200 m, schools, and railway stations <200 m. For the Probit models we report average marginal effects. Robust standard errors are clustered at the postcode and in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

information of shop owner name and shop owner type, which is available in the *Strabo* property dataset in order to test whether different ownership statuses yield different estimates of the effect of footfall on retail rents. As mentioned in Section 3.1, information on shop owner name is available for about one third of the sample that we use in the rent analysis. When this information is available, we count whether the owner rented out more properties in the same shopping street in our study period.

In columns (4)–(6) in Table D.5 we report the results using the interaction variable with footfall. In all specifications we do not find any statistically significant difference between single and multi-property owners. Columns (5)–(9) uses again two interaction terms with footfall; one that indicates whether the investor/owner is a private investor and the other whether the investor/owner is listed as real estate investor (*e.g.* real estate agencies, pension funds, construction companies etc.). The latter might internalise shopping externalities to some extent when the investor owns multiple properties in a shopping street or district. However, the coefficients of log footfall for private and real estate investors are virtually the same. We do not find statistically significant effects for the interaction effect. In columns (10)–(12) we include an interaction term with a dummy indicating whether a shop is part of an area-based retail association. These retail associations may offer public goods, such as advertisement for the shopping district and Christmas lightning, but they may also offer coordination strategies to reduce vacancies and, possibly, to internalise shopping externalities. However, we do not find evidence for the latter – the coefficients capturing the interaction effect is highly statistically insignificant.

Overall, the results in Table D.5 confirm that any coordination among property owners in order to internalise shopping externalities is very unlikely to happen in the setting of Dutch shopping streets.

In Table D.6 we replicate these results, but now use the number of shops within 200 m as a proxy for shopping externalities. We find very similar results. The only exception is that shopping externalities are more important when the owner is a private investor. This may indicate that internalisation by real estate investors may occur. However, the effect has the opposite sign once we use shops in 1832 as an instrument. Hence, we do not consider this as sufficient evidence that internalisation is important, in particular because all other interaction terms are highly statistically insignificant.

In Table D.7 we re-estimate the baseline results for the effects of number of shops on rents for different thresholds. The results highlight that the results are essentially unaffected when using 100 m or 250 m as thresholds rather than 200 m to count the number of shops in the vicinity. We note that we use the same threshold to construct the instruments.

D4. Sensitivity for vacancies

Here we investigate the robustness of the regressions where we aim to explain the impact of shopping externalities on the probability of a

Table D.9
Long-term and short-term vacancies.

(Dependent variable: shop is vacant)						
	Long-term vacancies			*Short-term vacancies*		
		Cinemas in 1930	*Shops in 1832*		*Cinemas in 1930*	*Shops in 1832*
Panel A: Footfall	(1) OLS	(2) 2SLS	(3) 2SLS	(4) OLS	(5) 2SLS	(6) 2SLS
Footfall *(log)*	−0.0145***	−0.0259***	0.0101	−0.0133***	−0.0212***	−0.0178***
	(0.0011)	(0.0052)	(0.0081)	(0.0007)	(0.0044)	(0.0066)
Shop and location characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Shopping street fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	332,304	332,304	176,407	371,049	371,049	197,144
R^2	0.0433			0.0127		
Kleibergen Paap F-statistic		82.50	39.98		82.66	42
Endogeneity test		5.299	10.93		3.419	0.563
$\xi^2(2)$ p-value		0.0213	0.000947		0.0645	0.453
Panel B: Number of shops	(1) OLS	(2) 2SLS	(3) 2SLS	(4) OLS	(5) 2SLS	(6) 2SLS
Number of shops, <200 m *(log)*	−0.0198***	−0.0223***	0.0092	−0.0192***	−0.0180***	−0.0163***
	(0.0019)	(0.0045)	(0.0072)	(0.0014)	(0.0038)	(0.0060)
Shop and location characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Shopping street fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	332,304	332,304	176,407	371,049	371,049	197,144
R^2	0.0407			0.0117		
Kleibergen Paap F-statistic		272.7	97.47		279.1	97.80
Endogeneity test		0.357	15.90		0.114	0.595
$\xi^2(2)$ p-value		0.550	6.69e-05		0.736	0.440

Notes: Footfall is measured as the number of shoppers per day. The construction year dummies are categorised as follows: <1832, 1832–1930, 1931–1950, 1951–1960, 1961–1970, 1971–1980, 1981–1990, 1991–2000, >2000. Location characteristics include whether the property is in a historic district, and the number of geocoded pictures <200 m, religious buildings <200 m, bus stops <200 m, public buildings <200 m, schools, and railway stations <200 m. Robust standard errors are clustered at the postcode and in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

shop being empty. In Table D.8 we consider some robustness checks with respect to the identification and estimation.

In columns (1)–(3) we consider an alternative identification strategy, where we only keep properties within 250 m of shopping street intersections and include street intersection fixed effects. Hence, rather than identifying the effects of shopping externalities within shopping streets, we now identify them between shopping streets. The results indicate that the effects of footfall and number of shops are very comparable to the baseline estimate. For the regressions using the number of shops in 1832 as an instrument for either footfall or number of shops < 200 m we now also find statistically significant coefficients, which is likely due to a stronger first stage.

In columns (4)–(6) of Table D.8, we consider an alternative explanation for the negative effect of shopping externalities on vacancies other than the effect of increased opportunity costs of vacant properties in expensive areas (*i.e.* with higher levels of footfall). In times of high demand, the marginal costs of providing store 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) (see *e.g.* Teulings et al., 2017). 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.[45]

[45] 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.

What we find is that the interaction term of shopping externalities and the dummy indicating whether the economy is in a bust is statistically significant and has the expected negative sign; hence, shopping externalities have a stronger impact on the probability to lie empty in bust periods because the marginal benefits are arguably lower. In column (4) we find, for example, that when footfall increases by 10% in boom times, the vacancy rate decreases by 0.27 percentage points. In bust times, this is 0.40 percentage points. The effect of the number of shops within 200 m is also stronger in bust times. The stronger effect in bust times survives once we instrument for shopping externalities in columns (5) and (6). 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 generally in line with the opportunity cost hypothesis: store 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 somewhat lower but still highly statistically significant. The only exception is when we instrument for shopping externalities with the number of shops in 1832 (see column (6)). However, given the relatively large standard error, we cannot exclude the possibility that there is a negative effect of shopping externalities on the probability to lie empty, which is in line with the previous results.

In columns (7)–(9) of Table D.8 we estimate Probit models, rather than Linear Probability Models. We note that in Probit models, fixed effects cannot be conditioned out, so we include dummies for shopping streets and years. Because in non-linear Probit models, marginal effects are not constant across observations we report average marginal effects. The results clearly show that the results are very similar to the corresponding baseline estimates reported in Tables 6 and 7, also when we instrument for shopping externalities. Hence, the estimation method does not seem to matter in measuring the impact of shopping externalities on the probability of a shop to lie empty.

In Table D.9 we consider the differential effect shopping externalities may have on long-term and short-term vacancy rates. We define long-term vacant properties a properties that lie empty for at least 3 years, while short-term vacant shops are defined as being non-vacant in the year before. We disregard all observations that are short-term vacant in the regressions where we explain the long-term vacancy rate and *vice versa*. It appears that, given our definitions, the average long-term vacancy rate in our sample is 1.6%, while the short-term vacancy rate is 3.3%.[46]

In columns (1)–(3) we investigate the impact of shopping externalities on the probability of a shop to remain empty for at least 3 years. The absolute magnitudes of the coefficients are smaller, but this is mainly due to a lower average probability to stay empty for a long time. The coefficient in column (1) suggest that the probability on long-term vacancy is reduced by 0.15 or 0.20 percentage points – about 10% of the mean long-term vacancy rate – when respectively footfall or the number of shops increases by 10%. Hence, this effect is about 50% stronger compared to the baseline estimate. This also holds if we instrument for shopping externalities with the number of cinemas in 1930 within 200 m. However, when using the number of shops in 1832 as an instrument, the coefficients are statistically insignificant. Hence, we find relatively stronger effects on the long-term vacancy rate if we use OLS or use the number of cinemas in 1930 as an instrument.

In columns (4)–(6) of Table D.9 we investigate the effects of shopping externalities on the short-term vacancy rate. In column (4), we find that a 10% increase in footfall or number of shops increases the probability of a shop to become empty by respectively 0.13 and 0.19 percentage points – about 5% of the mean short-term vacancy rate. Hence, the effects are very comparable to the baseline estimates. We now also find statistically significant negative impacts of shopping externalities on the short-term vacancy rate when using shops in 1832 as an instrument (column (6)).

All in all, we do not find large systematic differences in the way shopping externalities impact short-term and long-term vacancy rates, and generally both are reduced when footfall or number of shops increase in the vicinity of a shop.

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[46] Recall that the average probability of a shop to lie empty is 6.1%.