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House prices and accessibility: evidence from a quasi-experiment in transport infrastructure

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Abstract

This article studies the impact of accessibility on house prices based on a novel quasi-experiment in the Netherlands: the Westerscheldetunnel. We exploit the fact that the opening of the tunnel caused a major shift in accessibility for people and firms in the connected regions. Our results indicate that the accessibility elasticity of house prices is 0.8. We also find support for the idea of anticipation: about half of the accessibility effect already materializes more than 1 year before the opening of the tunnel. Finally, our analyses suggest that the impact of accessibility differs substantially across regions.

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JEL classifications: R2, R4
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1. Introduction

Accessibility is a key ingredient in the location decision of people and firms. The location decision of people is mainly based on the accessibility of jobs and amenities (Glaeser et al., 2001; Chen and Rosenthal, 2008). Improved accessibility, for instance as a result of new transport infrastructure, better enables people to work and live at a place that fits their skills and matches their needs (Teulings et al., 2014). For firms, accessibility lowers transportation costs and fosters agglomeration economies through *matching, sharing* and *learning* (Puga, 2010). Together, these factors often lead to clustering in economic centers (Krugman and Venables, 1995).

Despite the theoretical arguments to expect spatial economic effects of changes in accessibility, there is little consensus in the empirical literature (e.g. Gutiérrez et al., 2010). Some studies find evidence that transport infrastructure affects employment (Haughwout, 1999; Duranton and Turner, 2012), productivity (Pereira, 2000; Cantos et al., 2005), house prices (Gibbons and Machin, 2005; Klaiber and Smith, 2010; Levkovich et al., 2016) and population (Baum-Snow, 2007; Garcia-López et al., 2015). However, other studies report insignificant effects for the same indicators (on employment: e.g. Jiwattanakulpaisarn et al., 2009; on productivity: e.g. Garcia-Mila et al., 1996), or find that the impact of accessibility is negligible (Haughwout, 2002; Jiwattanakulpaisarn et al., 2011). Finally, another strand of the literature mainly identifies redistribution of economic activities and people due to changes in accessibility

(Chandra and Thompson, 2000; Moreno and López-Bazo, 2007; Redding and Turner, 2014).

One of the most salient reasons for the conflicting evidence is that studies in this field of research come across several empirical challenges. First, to obtain an observable impact of transport infrastructure, one needs to analyze a sufficiently large change in accessibility. This is problematic since substantial increases in accessibility are rare, given the existing dense network of roads and railways in most Western countries (Fernald, 1999; Banister and Berechman, 2001). Second, changes in accessibility are seldom exogenous (Duranton and Turner, 2012; Redding and Turner, 2014). It often remains unclear whether economic development results from improved infrastructure or the other way around. Particularly, investments in transport infrastructure are usually targeted to benefit areas with high or low economic growth (Garcia-López et al., 2015). This introduces the problem of reverse causality. Finally, the estimated relationship between spatial economic effects and changes in accessibility is frequently confounded by external developments in the area of research (Duranton and Turner, 2012; Baum-Snow and Ferreira, 2014).

This article aims to address these three issues by studying a quasi-experiment in the Netherlands: the Westerscheldetunnel. The key aspect that makes the Westerscheldetunnel a novel piece of transport infrastructure is that it exerted a substantial impact on accessibility since the Westerschelde estuary hampers traffic flows towards the other bank by nature. This is clearly illustrated by the 50% increase in the number of vehicles that crossed the estuary right after the tunnel opened and the (slower) ferry services closed down. The simultaneous abolishment of the ferries yields an even larger variation in accessibility due to the location of the tunnel: the ferries used to run on the east and west side of the estuary, while the tunnel is located in the center. This allows us to exploit both positive and negative changes in accessibility.

Second, the predominant goal of constructing the tunnel was not to promote economic growth of specific regions within the Dutch province of Zeeland. The main goal was to save on public costs as building and maintaining one tunnel would be less expensive in the long run than subsidizing two ferry lines (e.g. Priemus and Hoekstra, 2001; Louw et al., 2013). This makes the construction of the tunnel a rather exogenous event compared to the bulk of investments in transport infrastructure (see Section 2 for a more extensive discussion about this argument). Third, the existence of natural borders in the area under scope helps to limit the influence of external developments.

The main goal of this article is to estimate the effect of accessibility on house prices. To this end, this study employs detailed panel data at the postal code level for the period between 1995 and 2013 (the tunnel was opened in 2003). We prefer house prices as our variable of interest because house prices are able to absorb demand shocks rather quickly. Other indicators, such as population and employment growth, may be constrained by the pace of supply adjustment. Most importantly, when corrected for house characteristics using hedonic controls (Rosen, 1974), house prices can be used to evaluate residential land prices, which are a neat reflection of the attractiveness of regions. Hence, house prices function as an informative signal where (new) economic clusters will arise. A possible downside of using house prices to analyze the spatial impact of transport infrastructure is that the estimates may be confounded by supply adjustment. We argue, however, that this aspect plays at best a minor role in the area under scope (see Section 4.1 for a more detailed discussion on this point).

Additionally, this article addresses several other hypotheses on the accessibility capitalization into house prices. First, we examine the timing of capitalization by allowing for anticipation and delayed response. This analysis provides information about the rate at which people discount the future benefits of accessibility and their ability to predict the magnitude of the change in accessibility. Second, we explore whether the impact of accessibility differs across regions. Residents tend to spatially sort themselves into particular regions on the basis of socioeconomic status, and it is known that substantial heterogeneity exists in their valuation of travel time (Small et al., 2005). This resident heterogeneity hypothesis, and other potential explanations for regionally different capitalization patterns, are all put to the test.

The results show that accessibility positively and significantly affects house prices. On average, a 1% increase in accessibility leads to a 0.8% increase in house prices.[1] Moreover, about half of the accessibility effect already materializes more than 1 year before the opening of the tunnel. We do not find evidence for delayed response: the accessibility benefits of the tunnel were entirely capitalized in the year following the opening. Our analyses also suggest substantial heterogeneity between regions. While the northern region is likely to experience positive effects, the southern region does not seem to respond to the improved accessibility at all. The fact that these regions differ with respect to the characteristics of residents appears to be the most plausible explanation for the observed differences across regions.

The remainder of this article is structured as follows. Section 2 describes the quasi-experiment that we study. Section 3 presents the data and methodology. Section 4 reports the results, including a variety of sensitivity analyses. Section 5 concludes.

2. Quasi-experiment: the Westerscheldetunnel

This article focuses on the Dutch province of Zeeland, which is located in the southwestern part of the Netherlands (see Figure 1). The province covers almost 3000 km^2 and accommodates around 380,000 inhabitants (Statistics Netherlands, 2017). It mainly consists of islands and peninsulas in a delta area, and borders with Belgium in the South. Due to its geography, Zeeland is relatively isolated from the rest of the Netherlands though the Euclidian distance to the large cities of Rotterdam, Antwerp and Ghent is small. The relative isolation of Zeeland helps to limit the influence of external developments in the area under scope.

The province of Zeeland is generally perceived as part of the periphery of the Netherlands. The number of accessible jobs is relatively low (Meijers et al., 2013) and its population density is also low compared to the rest of the Netherlands (OECD, 2014). Additionally, Zeeland is relatively aged (Statistics Netherlands, 2017) and has a low population growth, urbanization rate and education level (OECD, 2014). Still, GDP per

1 For a different area of the Netherlands, Levkovich et al. (2016) estimate the accessibility elasticity of house prices to be about 1.76, using population as their measure of economic mass. Franklin and Waddell (2003) find that the elasticity depends on the sector, based on data from the State of Washington. The highest elasticity is obtained for the commercial sector (0.96); other sectors (university, industry and education) are characterized by an elasticity close to or sometimes even below zero. Iacono and Levinson (2011) conclude for the State of Minnesota that the elasticity equals 0.14, on average. Finally, evidence from Norway suggests an elasticity of 0.19 (Osland, 2010).

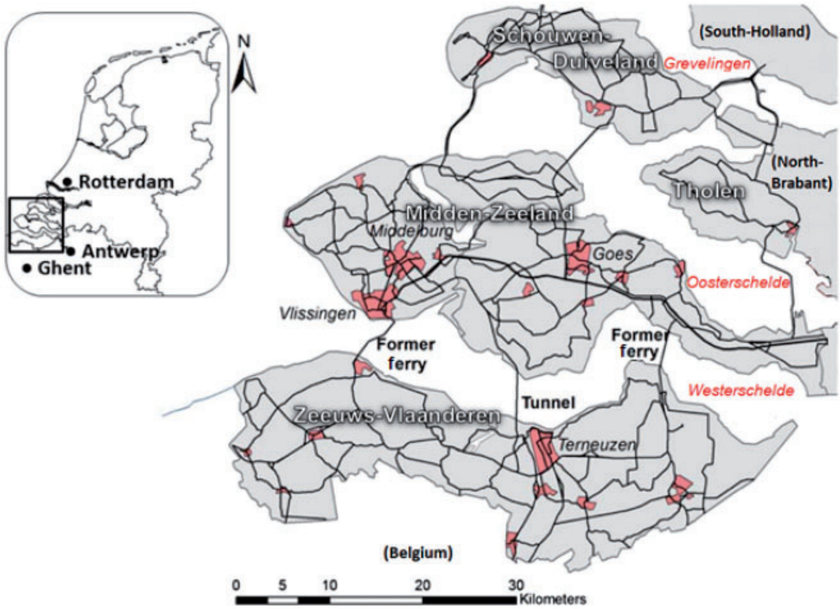

Figure 1. Detailed map of the province of Zeeland.
Source: Meijers et al. (2013), with slight adaptations.

capita and productivity rank medium, whereas its unemployment rate is among the lowest in the Netherlands (OECD, 2014).

The decision to construct the Westerscheldetunnel was made in September 1995 after almost a decade of uncertainty about the project due to fierce public debate, various feasibility studies and severe funding difficulties (Boekema, 2001; De Jong and Annema, 2011). The actual construction started in November 1997, whereas the tunnel was opened on 14 March 2003.[2] It connects Midden-Zeeland in the North to Zeeuws-Vlaanderen in the South. Before the opening of the tunnel, traffic had to use one of the ferry services. An alternative option was to drive through another tunnel near Antwerp in Belgium (not on the map in Figure 1) to cross the Westerschelde estuary.[3] Both ferries, one in the western and the other in the eastern part of the province of Zeeland, were closed on the day the tunnel was opened.[4] Hence, the simultaneous opening of the

2 The time period before March 2003 allows people to anticipate the opening and location of the tunnel, although projects of this scale and complexity usually involve a large degree of uncertainty with regard to both the final opening date and the exact route or location. We will test for anticipation more formally in Section 4.1.

3 There are no official figures indicating the number of vehicles that bypassed the ferry services via the Liefkenshoektunnel near Antwerp. Based on traffic counts, we do observe a decrease of about 2% in the number of vehicles that traveled through the Liefkenshoektunnel (comparing the year before and after the opening of the Westerscheldetunnel). However, this decrease is not statistically significant since traffic counts of the Liefkenshoektunnel are highly volatile. Moreover, we cannot rule out alternative explanations for this decrease. Also note that our accessibility measure, as will be discussed in Section 3.1, takes into account all alternative routes to access the southern region from other parts of the Netherlands (including routes via Belgium).

4 Nowadays, there still exists a small ferry service in the western part of the Westerschelde estuary, mainly for recreational use and restricted to pedestrians and cyclists.

Figure 2. Percentage change in accessibility due to the opening of the tunnel.
Note: Accessibility is measured as the number of accessible jobs, weighted by a generalized Gaussian distance decay function (see Section 3.1).

tunnel and abolishment of the ferries redirected trans-Westerschelde traffic from the outskirts towards the middle of the province. The changes in accessibility were particularly significant for the geographical center of both Midden-Zeeland and Zeeuws-Vlaanderen, and the outskirts of Zeeuws-Vlaanderen (see Figure 2). Other regions in Zeeland, such as Schouwen-Duiveland and Tholen, remained largely unaffected.

The time to cross the Westerschelde by ferry (including waiting time) was just under half an hour, while the tunnel can take cars across the estuary in 5 minutes. Traffic count numbers are illustrative for the substantial change in accessibility. In the first year after the opening of the tunnel, the number of vehicles that crossed the Westerschelde per working day increased by 50% and it has continued to rise by another 45% in the years afterwards (see Figure 3).[5] However, not all origin–destination combinations have experienced a travel time gain. For instance, mimicking the former ferry route by car through the tunnel now takes double the amount of time as before. This novel feature allows us to exploit substantial positive as well as negative changes in accessibility (see Figure 2).

As discussed before, empirical studies on the spatial impacts of transport infrastructure may be confounded by the problem of reverse causality. This problem arises because the bulk of investments in transport infrastructure is aimed to facilitate

5 Capacity constraints have been of no concern in the situation of the ferries as well as after the opening of the tunnel. Using detailed information on the ferry services (Provinciale Stoombootdiensten in Zeeland, n.d.) and a rule of thumb for road capacities (Grontmij, 2015), we approximate the maximum daily capacity of the ferries and the tunnel to be 22,000 and 200,000 vehicles, respectively. Anecdotal evidence from representatives of the province of Zeeland corroborates that capacity constraints have never been prominent for the ferry services.

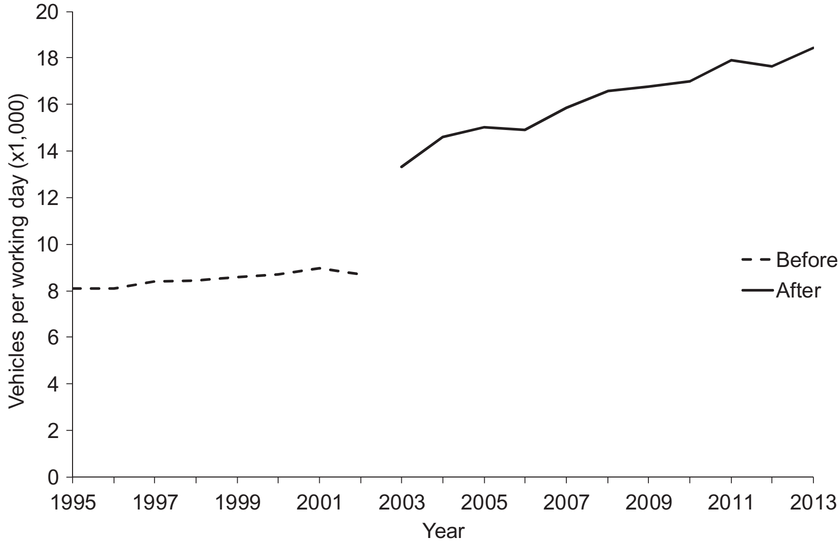

Figure 3. Traffic counts across the Westerschelde estuary per working day.
Source: Own calculations using data from the province of Zeeland.

expected economic growth in the future, which makes it difficult to identify the direction of causation. Although it has been argued that the Westerscheldetunnel would promote economic growth at a large spatial scale, i.e. the entire province of Zeeland, it has never been a prominent goal to support specific regions within this province. Since the spatial scope of this article comprises Zeeland only, and excludes surrounding regions, this alleviates concerns that the results are subject to reverse causality.

Moreover, it should be noted that economic growth was not the dominant goal to build this tunnel. Instead, the most important argument was to save on public costs, as building and maintaining one tunnel was less expensive in the long run than subsidizing two ferry services (e.g. Priemus and Hoekstra, 2001; Louw et al., 2013). More specifically, the Dutch government in power at that time had to decide between either spending 24 million euros per year on the ferries for an indefinite period of time, or utilizing the same amount of money for an expected period of 30 years to finance the construction of the tunnel (with considerably lower costs of personnel and maintenance). The latter option was chosen and eventually the actual costs of building the tunnel, which amounted to 750 million euros, only marginally exceeded the expected costs (De Jong and Annema, 2011). Another, more subordinate goal was to promote safety on the Westerschelde estuary since the ferries were crossing the busy shipping lane to the international port of Antwerp (Van Stralen, 1990). From a technical perspective, the project was also expected to deepen engineers' knowledge on tunnel construction in soft clay ground (Heijboer et al., 1999).

In line with the cost considerations discussed above, the decision about the location of the tunnel was based on finding a proper substitute that allowed the abolishment of both the eastern and western ferry service, while limiting the loss in accessibility for regions close to the ferry crossings (Van Stralen, 1990; Kooijman, 1996). Hence, from this perspective, the middle of the Westerschelde estuary was found to be the only sensible location. This helps to ensure that the role of confounding factors—potentially influencing the relationship between accessibility and house prices—is limited. Nevertheless, given that the tunnel had to be placed somewhere in the middle of the estuary, the exact location was determined by two important factors. First, the route of the tunnel and corresponding on-ramps had to connect to the existing transport network and, most importantly, to the road in the direction of Belgium (Rijkswaterstaat, 1997; Allaert, 2001). Second, the final route had to circumvent a protected region of natural and cultural–historical importance in the most southern part of Midden-Zeeland (Kooijman, 1996; Boekema, 2001). All in all, we are confident that the quasi-experiment in this study helps to avoid most of the endogeneity problems that often hinder studies in this field of research.

3. Methodology and data

3.1. Identification strategy

The impact of improved infrastructure on economic outcomes is often analyzed using a difference-in-differences framework (e.g. Gibbons and Machin, 2005; Billings, 2011; Ghani et al., 2013). This approach requires a valid control region that has not been affected by the change in accessibility. However, in our setting the opening of the tunnel coincided with the abolishment of the ferry services, which causes that all areas in the province of Zeeland experienced an accessibility change (see Figure 2). This rules out the possibility of a proper control region that acts as a counterfactual and, hence, the use of a difference-in-differences strategy.[6]

In this study we take an alternative approach by relying on the exogenous nature of the Westerscheldetunnel, as discussed in Section 2, and the inclusion of a wide variety of control variables. We identify the impact of a change in accessibility on house prices using postal code fixed effects and (hedonic) control variables for house characteristics, in line with Gibbons and Machin (2005).[7] The aim of the postal code fixed effects is to curb endogeneity problems related to time-invariant postal code characteristics, whereas hedonic controls are included to correct for house characteristics that may vary across regions and over time. The hedonic controls are an important part of our identification strategy, because they reveal information about residential land prices and thus the attractiveness of regions.

Additionally, omitted time-varying characteristics of regions potentially relevant for house prices, such as crime rates or the provision of public goods, may be correlated with the change in accessibility. For instance, bias could arise if the provision of public goods—possibly influenced by local or regional planning policies—evolves differently in regions close to the tunnel than in regions located further away. However, the Westerscheldetunnel's quasi-experimental setup and the relative remoteness of the area under scope alleviate most concerns with regard to confounding variables. Moreover, in order to address remaining bias arising from time-variant factors, our regression specifications include postal code specific linear time trends.

6 Control regions outside the province of Zeeland involve serious concerns with regard to the common trend assumption underlying a difference-in-differences strategy.

7 A Hausman test was conducted to differentiate between a fixed effects and a random effects model. The results from this test reject a random effects model ($Chi^2 = 5650.67$, p-value = 0.0000).

Together, the postal code fixed effects, hedonic controls, postal code specific linear time trends and the exogenous nature of the Westerscheldetunnel, give us confidence that the model specification below is informative about the causal effect of accessibility on house prices. The first regression equation to be estimated in Section 4.1 is:

$$\ln P_{iztm} = \alpha + \theta \ln A_{zt} + \gamma \boldsymbol{X}_{it} + \delta_y^I \boldsymbol{Y} + \delta_m^{II} \boldsymbol{M} + \delta_z^{III} \boldsymbol{Z} + \rho_z y_t + \epsilon_{iztm}, \tag{1}$$

where P_{iztm} denotes the house transaction price of dwelling i in postal code z, at year t, in month m.[8] A_{zt} indicates the accessibility for postal code z and year t with accessibility elasticity of house prices θ. $\boldsymbol{X}_{it}$ is a vector of hedonic control variables that represent house characteristics at the level of the individual house (see Appendix A for detailed information). $\boldsymbol{Y}$, $\boldsymbol{M}$ and $\boldsymbol{Z}$ are vectors of year, month and postal code fixed effects, with δ_y^I, δ_m^{II} and δ_z^{III} as their estimated coefficients. y_t is a linear scale variable that denotes the year of house sale ($y_{1995} = 1$, $y_{1996} = 2 \ldots$) and its effect ρ_z differs per postal code z. ϵ_{iztm} reflects a random error term clustered at the postal code level.[9]

The potential accessibility framework that we employ in Equation (1) focuses on the *possibility* to access economic mass, a collective term for jobs, people and amenities (e.g. Van Wee et al., 2001; Gutiérrez et al., 2010; Donaldson and Hornbeck, 2016). The general equation for potential accessibility equals

$$A_{zt} = \sum_{d=1}^{D} E_d F(\tau_{zdt})$$

where d is an index that relates to destinations that one can travel to from postal code z. Foreign areas (such as in Belgium) as well as the postal code area z itself are also part of the set of D feasible destinations. E_d captures the economic mass in destination d at the date the tunnel was opened; we measure this using the number of jobs.[10] τ_{zdt} describes the travel time required to reach destination d from postal code z at date t. Subsequently, travel time is used as an input for our generalized Gaussian distance decay function $0 \leq F(.) \leq 1$, based on the estimates of De Groot et al. (2015) for people's willingness to commute τ minutes in the Netherlands (see the main estimation in Figure 4). The idea is that jobs located further away get increasingly smaller weights, until the weight becomes 0 for jobs located more than 90 minutes of travel time away (one-way trip). To explore the robustness of our preferred accessibility measure, we also employ other cutoff values of the Gaussian distance decay function (60 and

8 We adopt a log–log specification because we expect the effect of accessibility on house prices to be proportional rather than additive. This also allows us to interpret the estimated coefficient as an elasticity. Nevertheless, similar results are obtained if we employ a log–lin specification.

9 Clustered error terms correct for the spatial autocorrelation that arises because accessibility is measured at the postal code level (Angrist and Pischke, 2009), while house prices are measured at the level of the individual house (Moulton, 1990).

10 E_d is not allowed to vary over time since relocation of economic mass may be a generative effect of the tunnel and should, therefore, be absorbed by the house prices (rather than controlling for it in the accessibility measure). Nevertheless, if we do allow E_d to vary over time, we might be able to indirectly assess whether or not relocation of economic mass has occurred. The results from this exercise closely resemble the results in which E_d is not allowed to vary over time. If anything, this indirect evidence suggests that the opening of the tunnel did not trigger substantial relocation.

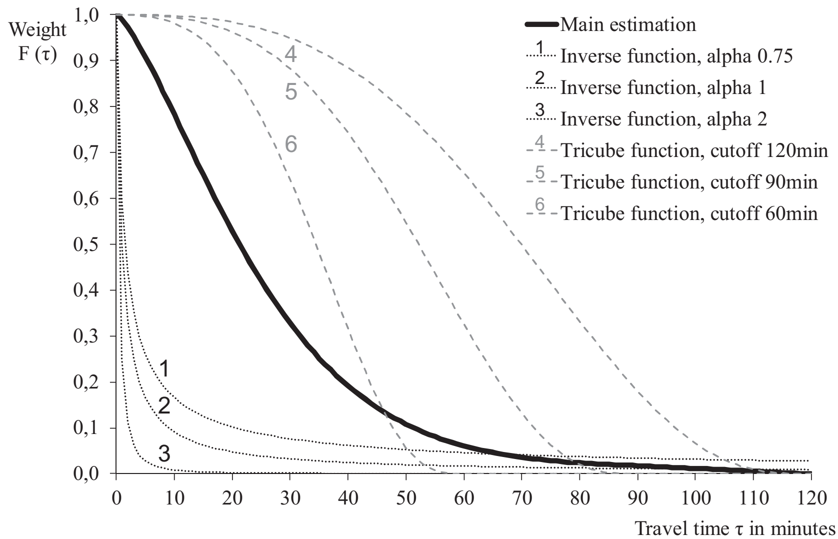

Figure 4. Distance decay functions.

120 minutes of travel time) and alternative distance decay functions (see Figure 4 and Section 4.2).[11]

To allow for anticipation effects, i.e. future accessibility benefits that already capitalize in house prices before the opening of the tunnel (McDonald and Osuji, 1995), we include an additional term and estimate the following equation:

$$\ln P_{iztm} = \alpha + \theta \ln A_{zt} + \theta_{ant}\omega_t \ln\left(\frac{A_{z,after}}{A_{z,before}}\right) + \gamma \boldsymbol{X}_{it} + \delta_y^I \boldsymbol{Y} + \delta_m^{II} \boldsymbol{M} + \delta_z^{III} \boldsymbol{Z} + \rho_z y_t + \epsilon_{iztm} \tag{2}$$

The additional term in Equation (2) reflects the relative change in accessibility due to the tunnel in postal code z (see Ossokina and Verweij, 2015). If people anticipate an accessibility gain, house prices will start to respond to this before March 2003. ω_t is a vector of four dummy variables that equal 1 for, respectively, 2000, 2001, 2002 and 2003 (before 14 March).[12] θ_{ant} measures the degree of capitalization in these years compared

11 A potential source of concern is related to classical measurement error since the true stock of infrastructure relevant for our accessibility variable might be measured inaccurately. This measurement error would bias the coefficients toward zero and, hence, the measured effect of accessibility on house prices would be an underestimate. Yet, we argue that this is of limited importance in our setting. Most importantly, the regressions include postal code fixed effects, which alleviates concerns related to mismeasurement of the time-invariant infrastructure stock. The potential measurement error that remains, i.e. the time-varying part of the stock of infrastructure, is unlikely to play a decisive role since the Westerscheldetunnel was the only large-scale infrastructure project in the province of Zeeland during our sample period. According to representatives of the province of Zeeland, all other projects such as the (re)construction of some parallel roads and roundabouts, were small-scaled and only very modestly affected accessibility.

12 We select the year 2000 as the first year that anticipation effects may take place. In what follows, we show that this choice is unlikely to drive the results, as anticipation does not start until the year 2002.

to the baseline period before the year 2000. Again, each of the four estimates of θ_{ant} can be interpreted as an elasticity.

One might also argue that θ increases (decreases) over time. For instance, people may gradually learn about the benefits of the tunnel. This delayed response hypothesis implies that θ, which estimates the average house price effect of a change in accessibility, overestimates (underestimates) the effect in the first years after the opening of the tunnel, and underestimates (overestimates) it for later years. In that case, the delayed response effect shows up in the error term. An obvious way to test the delayed response hypothesis would be to include additional terms equivalent to those used to test the anticipation hypothesis. However, this approach would be problematic: including an additional term for every year after the opening of the tunnel introduces the problem of multicollinearity since we also include a full set of year dummies and postal code specific linear time trends. Therefore, we test the delayed response hypothesis by including one additional term that captures delayed response effects during the period 2008–2013. The coefficient of this term reflects the effect of the accessibility change on house prices during 2008–2013 compared to the period 2003–2007.[13]

$$\ln P_{iztm} = \alpha + \theta \ln A_{zt} + \theta_{del}\omega_{2008-2013}\ln\left(\frac{A_{z,after}}{A_{z,before}}\right) + \gamma \boldsymbol{X}_{it} + \delta_y^I \boldsymbol{Y} + \delta_m^{II}\boldsymbol{M} + \delta_z^{III}\boldsymbol{Z} + \rho_z y_t + \epsilon_{iztm} \tag{3}$$

Additionally, people may both anticipate the opening of the tunnel and subsequently learn about the true benefits once the tunnel has become operational. If this is the case, then the previous equations yield biased estimates. To this end, we estimate an equation that includes terms for both anticipation and delayed response effects:

$$\begin{aligned}\ln P_{iztm} = {} & \alpha + \theta \ln A_{zt} + \theta_{ant}\omega_t \ln\left(\frac{A_{z,after}}{A_{z,before}}\right) + \theta_{del}\omega_{2008-2013}\ln\left(\frac{A_{z,after}}{A_{z,before}}\right) + \gamma \boldsymbol{X}_{it} + \delta_y^I \boldsymbol{Y} + \delta_m^{II}\boldsymbol{M} \\ & + \delta_z^{III}\boldsymbol{Z} + \rho_z y_t + \epsilon_{iztm}\end{aligned} \tag{4}$$

The methodological framework of this article is schematically summarized in Figure 5. Equation (1) assumes that the benefits of the tunnel fully capitalize right after the opening of the tunnel, whereas Equation (2) supposes that capitalization already takes place before. Note that Equation (1) underestimates the impact of the tunnel if anticipation exists since it treats the anticipation period as part of the before period, reducing the before–after difference. If the anticipation period is deleted from the sample, Equations (1) and (2) will therefore yield similar results. Equation (3) allows for delayed response and its average treatment effect is, by construction, equal to that of Equation (1). Finally, Equation (4) combines both the anticipation and delayed response effects.

The impact of the tunnel may differ across regions. To this end, we estimate the relevant equations including an interaction effect with a region dummy that equals 1 for observations located to the south of the Westerschelde estuary (Zeeuws-Vlaanderen), and 0 otherwise (the northern region). In addition, we explore potential mechanisms

13 Note that this is also the reason why we do not test for delayed response during 2003–2007. Such a regression would be equivalent to Equation (3) with the only difference being that the coefficient for delayed response will switch sign.

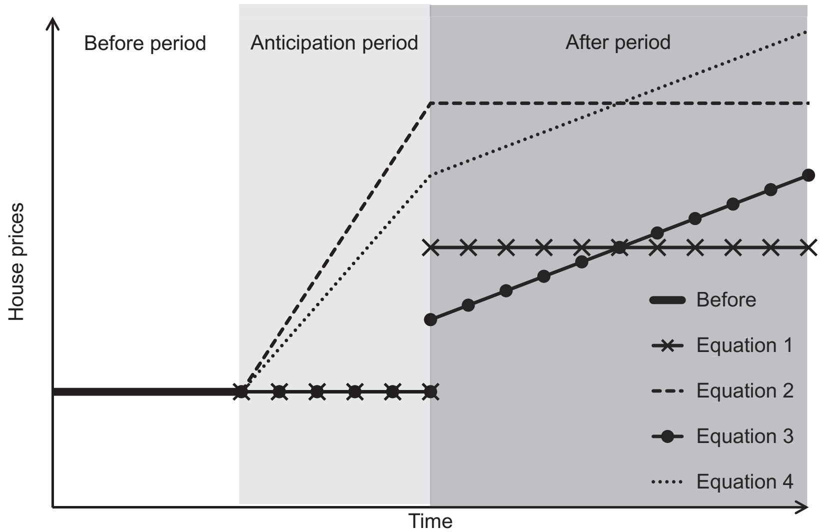

Figure 5. Methodological framework (for a postal code with an increase in accessibility). *Note*: The methodological framework for a postal code with a decrease in accessibility is the mirror image.

underlying regionally different responses to a change in accessibility. We will elaborate further on this in Section 4.1.

3.2. Accessibility data

Our data on travel time and economic mass (in terms of employment) stem from the input database of the leading regional transport model in the Netherlands: Nederlands Regionaal Model Zuid (NRM Zuid). The model is widely applied for benefit–cost analyses and audited on a regular basis to ensure accuracy. The input database combines observational data from several sources, such as the Landelijk Informatiesysteem van Arbeidsplaatsen (LISA) employment database that registers all jobs in the Netherlands at the local level. Our dataset contains data on 3300 areas, both Dutch and foreign, including the travel time between all of these areas by car.

The NRM Zuid model is also able to create a counterfactual travel time matrix: the travel times that apply to the situation before the opening of the tunnel. To this end, we erase the tunnel and corresponding on-ramps from the transport network in the model and reintroduce the ferry services (including an average waiting time of 7 minutes). The model then calculates the counterfactual behavior of road users in terms of destination and route choice, based on the new generalized travel costs and revealed preferences in the model's base traffic network. The counterfactual network and road user behavior together determine the counterfactual travel time matrix and, hence, accessibility (we leave economic mass E_d unchanged).

The 3300 areas in the NRM Zuid model are smaller than postal codes. We need to aggregate these areas to (the size of) postal codes, to ensure that there are enough observations of house transactions per geographical subdivision. We aggregate accessibility data using weighted averages of the number of commuting trips in an

Table 1. Descriptive statistics of accessibility

Region	Number of postal codes	Mean accessibility before tunnel, number (travel time weighted) jobs	Mean increase in accessibility	Minimum increase in accessibility	Maximum increase in accessibility	SD of percentage accessibility increase
Zeeland	153	176,706	6.80%	−5.22%	25.80%	5.78%
Northern region	102	165,170	7.92%	0.04%	25.80%	4.88%
Southern region	51	199,778	4.55%	−5.22%	22.96%	6.72%

area. This results in 934 postal codes with data on accessibility before and after the opening of the tunnel of which 153 are located in the province of Zeeland. The change in accessibility for these 153 postal codes is shown in Figure 2.

Table 1 presents descriptive statistics of accessibility and the substantial shift caused by the tunnel. The largest increase in accessibility is more than 25%, while other regions experience a decrease of about 5%. These percentages correspond to approximately 46,000 extra (travel time weighted) jobs and 9,000 fewer jobs accessible, respectively. The average postal code in the province of Zeeland was able to access almost 12,000 extra jobs. In the southern region, potential accessibility is highest before the opening of the tunnel due to the proximity of Antwerp and Ghent in Belgium. On average, the northern region experiences the largest increase in accessibility. Nevertheless, the southern region shows the largest variation in accessibility (as indicated by the standard deviation).

3.3. House price data

We use micro data on house prices from the administrative database of the Dutch Association of Real Estate Brokers and Experts (NVM). Almost half of the real estate brokers in the province of Zeeland are members of the NVM.[14] In sum, the dataset contains 38,948 house transactions, including the date of sale, transaction price and a variety of house characteristics (see Appendix A for an overview), for the period between 1985 and 2013. A total of 27,835 observations in 146 postal codes remains after removing incomplete observations and restricting the sample to 1995 and onwards.[15] Appendix B describes this selection procedure in more detail. Table 2 provides the number of house transactions and prices on a year-to-year basis.

Figure 6 shows the development of average house prices per square meter for the northern and the southern region (Zeeuws-Vlaanderen). The graph indicates a steady increase in house prices over the years until the crisis (from 2008 onwards). House prices in the northern region are about 25% higher than in the southern region. There are no

14 A comparison of the average price of sold housing in the NVM database and that of Statistics Netherlands, which has almost complete coverage, provides confidence that the house transactions performed by NVM real estate brokers are representative.

15 We have removed 7 postal codes due to insufficient house price observations (<13 during the whole period). Similar results are obtained if we use different thresholds for the minimum number of house transactions in a postal code (see Appendix C).

Table 2. Number of house transactions and transaction prices per year in the province of Zeeland

Year	Number of transactions	Mean transaction price in euro	SD of transaction price in euro	Minimum transaction price in euro	Maximum transaction price in euro[a]
1995	715	89,162	42,163	22,689	298,587
1996	940	95,010	53,172	22,689	589,914
1997	1093	97,546	49,282	24,958	492,352
1998	1153	107,876	56,537	24,958	621,679
1999	1355	119,974	63,756	29,496	544,536
2000	1488	132,292	75,161	22,689	689,746
2001	1820	144,000	81,704	22,689	703,360
2002	1780	166,530	88,144	25,000	699,100
2003	1738	184,712	94,189	33,000	706,024
Before tunnel	323	174,808	91,242	44,111	612,603
After tunnel	1415	186,973	94,735	33,000	706,024
2004	1750	192,345	102,339	31,000	710,000
2005	2006	203,978	101,348	32,000	710,000
2006	2014	214,108	109,557	35,000	710,000
2007	1948	215,164	107,386	32,500	700,000
2008	1622	212,806	105,167	34,000	687,500
2009	1230	199,326	98,559	37,500	710,000
2010	1219	204,557	102,455	47,500	700,000
2011	1192	200,287	101,494	34,000	710,000
2012	1,388	192,464	103,022	29,000	700,000
2013	1,384	190,129	99,167	22,500	675,000
Whole sample	27,835	173,446	100,330	22,500	710,000

[a]In five different years, the maximum house price is equal to 710,000 euros. These five transactions all represent different houses.

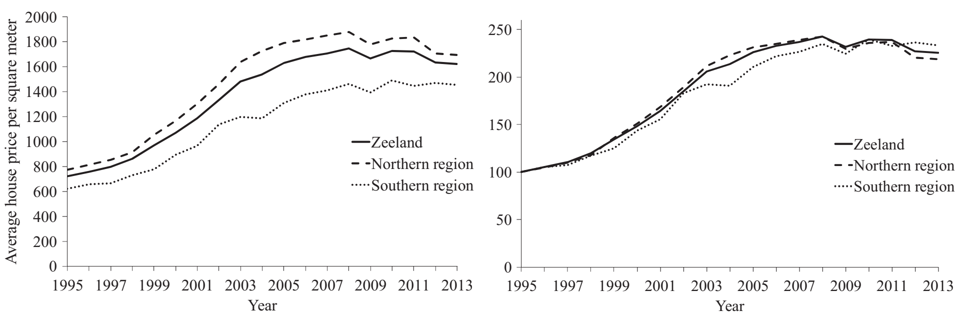

Figure 6. Average house prices per square meter in euro (left) and indexed (right, 1995 = 100).

systematic differences in the trend across regions before the opening of the tunnel in 2003. However, from 2003 onwards the southern region appears to lag somewhat behind the northern region. This may be related to the fact that the average accessibility increase in the southern region was lower (see Figure 2) or because the southern region responds less strongly to an increase in accessibility. We will test this more formally in the next section.

Table 3. Effect of accessibility on house prices

	Equation (1)	Equation (2)	Equation (3)	Equation (4)
$\theta_{ant}\omega_{2000}$		0.033		0.095
		(0.161)		(0.151)
$\theta_{ant}\omega_{2001}$		−0.031		0.053
		(0.173)		(0.158)
$\theta_{ant}\omega_{2002}$		0.427*		0.534***
		(0.218)		(0.120)
$\theta_{ant}\omega_{2003}$		0.599**		0.744***
		(0.271)		(0.244)
θ	0.484***	0.790***	0.465***	0.944***
	(0.131)	(0.277)	(0.111)	(0.245)
$\theta_{del}\omega_{2008-2013}$			−0.064	0.177
			(0.146)	(0.111)
N (postal codes)	27,835 (146)	27,835 (146)	27,835 (146)	27,835 (146)
Within R^2	0.869	0.869	0.869	0.869

All results are based on postal code fixed effects regressions. Standard errors are clustered at the postal code level (in parentheses). Year fixed effects, month fixed effects and hedonic controls for house characteristics are included as well as postal code specific linear time trends.
*/**/***denote significance at the 10%, 5% and 1% level.

4. Results

4.1. Main findings

Table 3 shows the results for all equations described in Section 3.1.[16] Equation (1) finds a positive and significant elasticity of 0.484, which implies that a 1% increase in accessibility leads to an increase in house prices of around 0.5%.[17] The estimation results of Equation (2) show that anticipation starts from 2002 and increases as the opening of the tunnel approaches.[18] The results also reveal that the accessibility elasticity of house prices based on Equation (1) is probably an underestimate. When including anticipation terms, the effect accumulates to 0.8% for an accessibility gain of 1%. This is intuitive: if one ignores anticipation while it does exist, part of the accessibility effect is assigned to the pre-tunnel period, yielding a smaller difference between the period before and after the opening of the tunnel (see Figure 5).

Equation (3) tests the hypothesis of delayed response, while ignoring anticipation, and does not find evidence in favor of delayed response. Delayed response is again

16 Table 3 only contains the estimates of the variables of interest, i.e. the variables that reflect accessibility. The estimates of all other (hedonic) control variables give the expected result and can be found in Appendix A.

17 Without postal code specific linear time trends, the elasticity is around zero and insignificant. This is because the linear time trends and the accessibility increase are negatively correlated (−0.30). Hence, the change in accessibility would be endogenous when omitting the postal code specific linear time trends. Section 4.2 provides a more detailed discussion on this point.

18 A finer-grained measure using 13 quarterly anticipation variables (for the first quarter of 2000 until the first quarter of 2003) indicates a smooth increase of house prices throughout this period (see Appendix D).

Table 4. Effect of accessibility on house prices with regional interaction

	Baseline estimation from Table 3	With regional interaction
$\theta_{ant}\omega_{2000}$	0.033	0.219
	(0.161)	(0.230)
$\theta_{ant}\omega_{2001}$	−0.031	0.279
	(0.173)	(0.221)
$\theta_{ant}\omega_{2002}$	0.427*	0.781***
	(0.218)	(0.236)
$\theta_{ant}\omega_{2003}$	0.599**	1.077***
	(0.271)	(0.273)
θ	0.790***	1.497***
	(0.277)	(0.257)
$\theta_{ant}\omega_{2000}D_{z,South}$		−0.363**
		(0.179)
$\theta_{ant}\omega_{2001}D_{z,South}$		−0.578***
		(0.163)
$\theta_{ant}\omega_{2002}D_{z,South}$		−0.661***
		(0.180)
$\theta_{ant}\omega_{2003}D_{z,South}$		−0.733***
		(0.240)
$\theta D_{z,South}$		−1.317***
		(0.202)
N (postal codes)	27,835 (146)	27,835 (146)
Within R^2	0.869	0.870

All results are based on postal code fixed effects regressions. Standard errors are clustered at the postal code level (in parentheses). Year fixed effects, month fixed effects and hedonic controls for house characteristics are included as well as postal code specific linear time trends.
*/**/***denote significance at the 10%, 5% and 1% level.

found to be insignificant when taking anticipation into account, see the results of Equation (4). If anything, house prices in regions that experienced an increase in accessibility were, conditional on the effect that capitalized right after the opening of the tunnel, marginally higher during 2008–2013. In sum, we conclude that the benefits were fully capitalized into house prices in the year the tunnel was opened. All estimates in the remainder of this article will, therefore, be based on Equation (2).[19]

4.1.1. Differences across regions

To examine whether the northern region (Midden-Zeeland, Tholen and Schouwen-Duiveland) and the southern region (Zeeuws-Vlaanderen) have reacted similarly to a change in accessibility, we interact the accessibility measure with a dummy variable that equals 1 if the postal code is part of the southern region, and 0 otherwise ($D_{z,South}$). Table 4 shows that the positive effect of accessibility on house prices is likely to be driven by the northern region. In this region, house prices increase by 1.5% for every 1% increase in accessibility. The southern region hardly experiences any observable

19 All results in Table 3 are robust to the use of an accessibility measure based on population rather than jobs (see Appendix E).

Table 5. Effect of accessibility on house prices with interactions for initial accessibility, education level and postal code fixed effects

	With initial accessibility interaction ($D_{z,High\_acc}$)	With education interaction ($D_{z,High\_edu}$)	With postal code fixed effects interaction ($D_{z,High\_FE}$)
$\theta_{ant}\omega_{2000}$	0.055	0.074	0.039
	(0.192)	(0.170)	(0.173)
$\theta_{ant}\omega_{2001}$	0.015	0-.007	−0.088
	(0.199)	(0.176)	(0.172)
$\theta_{ant}\omega_{2002}$	0.383	0.393*	0.345
	(0.248)	(0.219)	(0.220)
$\theta_{ant}\omega_{2003}$	0.684**	0.537**	0.514*
	(0.300)	(0.254)	(0.260)
θ	0.953***	0.525**	0.480*
	(0.281)	(0.258)	(0.262)
$\theta_{ant}\omega_{2000}D_z$	−0.051	−0.172	−0.049
	(0.166)	(0.166)	(0.176)
$\theta_{ant}\omega_{2001}D_z$	−0.115	−0.097	0.134
	(0.155)	(0.164)	(0.165)
$\theta_{ant}\omega_{2002}D_z$	0.081	0.075	0.199
	(0.174)	(0.177)	(0.185)
$\theta_{ant}\omega_{2003}D_z$	−0.224	0.250	0.323
	(0.217)	(0.254)	(0.244)
θD_z	−0.406*	0.715***	0.745***
	(0.242)	(0.210)	(0.227)
N (postal codes)	27,835 (146)	27,835 (146)	27,835 (146)
Within R^2	0.869	0.870	0.869

All results are based on postal code fixed effects regressions. Standard errors are clustered at the postal code level (in parentheses). Year fixed effects, month fixed effects and hedonic controls for house characteristics are included as well as postal code specific linear time trends.
*/**/***denote significance at the 10%, 5% and 1% level.

effect with an insignificant accessibility elasticity of (1.497 − 1.317 =) 0.180. A similar pattern holds for the anticipation effects. Hence, our analyses indicate substantial heterogeneity between regions.

To explore potential mechanisms for the regionally different response to a change in accessibility, we conduct several analyses. First, we examine whether the north–south difference might be the result of a nonlinearity in the relationship between accessibility and house prices. For instance, a positive change in accessibility may have a larger effect on house prices if the initial accessibility level is relatively low, whereas this effect might be smaller if initial accessibility is already at a high level. Note that the accessibility of jobs was approximately 21% larger in the southern region than in the northern region before the opening of the tunnel (see Table 1).

We put this idea to the test by replacing the regional dummy ($D_{z,South}$) with an accessibility dummy that equals 1 if the postal code had an initial accessibility level larger than the median postal code, and 0 otherwise ($D_{z,High\_acc}$). The results from this analysis, presented in the first column of Table 5, indicate that the interaction terms of the anticipation effects are all insignificant. However, the interaction term of the main effect is marginally significant with a *p*-value of 0.096 and has a negative sign in line with the nonlinearity hypothesis. In sum, we conclude that there is some evidence in favor of a nonlinear relationship between accessibility and house prices though this effect is unlikely to fully explain the large north–south difference in Table 4.

An alternative explanation for the differences across regions is related to heterogeneity among residents. As shown by Small et al. (2005), substantial heterogeneity exists in how people value travel time. To test this idea of resident heterogeneity, we use cross-section data on educational attainment at the municipality level (from Statistics Netherlands) and create an education dummy that equals 1 if the postal code belongs to a relatively highly educated municipality, and 0 otherwise ($D_{z,High\_edu}$).[20] Subsequently, this variable is again interacted with the accessibility variables. The results in the second column of Table 5 show that highly educated municipalities have responded significantly stronger to the change in accessibility. This suggests that the weak response in the southern region (with on average a lower education level than in the northern region) may be caused by the relatively low educational attainment of its residents.

Another strategy to address heterogeneity among residents is to use the estimated postal code fixed effects as an indicator of the residential land prices in the different postal codes.[21] This approach reveals valuable information about the income of households and their willingness to pay for (attractive) residential housing. Similar to the previous analyses, we construct an interaction term using a dummy variable that equals 1 if the postal code's fixed effect is larger than the median value, and 0 otherwise ($D_{z,High\_FE}$). It follows from the third column of Table 5 that postal codes with a relatively large postal code fixed effect, i.e. households with a relatively high income and/or high willingness to pay, have responded more strongly to the change in accessibility.[22] Again, this analysis provides evidence in favor of the resident heterogeneity hypothesis.

In addition, we have explored housing supply as a potential mechanism for the regionally different capitalization pattern. However, the construction of new houses is unlikely to have caused the observed differences across regions. First, the growth of the housing stock in the province of Zeeland was reasonably below the national average during the period 1995–2011 (13% and 16%, respectively).[23] Second, the limited supply response is not due to supply restrictions: zoning restrictions are seldom binding in

20 A municipality is considered to be highly educated if at least 25% of the workforce has a university degree and at most 25% did not complete intermediate vocational education.

21 Note that house prices consist of a building and a land component. Since our estimation framework contains hedonic control variables for house characteristics, it is possible to use the postal code's fixed effect as an indicator of the postal code's residential land price.

22 In fact, when using a finer-grained division of postal code fixed effects into tertiles or quartiles, it appears that the response to a change in accessibility is increasing in the value of the postal code's fixed effect (see Appendix F).

23 To ensure that this factor plays a minor role, we have analyzed whether municipalities with an above average growth of the housing stock influence the estimates. The results from this exercise do not reveal systematic differences compared to the main results of Table 3.

Table 6. Robustness to different cutoff values for the Gaussian distance decay function

	Baseline estimation (cutoff 90)	Cutoff 60	Cutoff 120	Baseline with regional interaction (cutoff 90)	Cutoff 60 with regional interaction	Cutoff 120 with regional interaction
$\theta_{ant}\omega_{2000}$	0.033	−0.151	−0.016	0.219	−0.086	0.267
	(0.161)	(0.124)	(0.144)	(0.230)	(0.165)	(0.266)
$\theta_{ant}\omega_{2001}$	−0.031	−0.222*	−0.114	0.279	−0.095	0.343
	(0.173)	(0.125)	(0.155)	(0.221)	(0.161)	(0.258)
$\theta_{ant}\omega_{2002}$	0.427*	0.133	0.306	0.781***	0.263	0.935***
	(0.218)	(0.159)	(0.222)	(0.236)	(0.175)	(0.277)
$\theta_{ant}\omega_{2003}$	0.599**	0.295	0.404	1.077***	0.457**	1.285***
	(0.271)	(0.196)	(0.284)	(0.273)	(0.217)	(0.320)
θ	0.790***	0.441**	0.564*	1.497***	0.774***	1.771***
	(0.277)	(0.184)	(0.294)	(0.257)	(0.198)	(0.302)
$\theta_{ant}\omega_{2000}D_{z,South}$				−0.363**	−0.220	−0.386*
				(0.179)	(0.146)	(0.211)
$\theta_{ant}\omega_{2001}D_{z,South}$				−0.578***	−0.377***	−0.607***
				(0.163)	(0.131)	(0.194)
$\theta_{ant}\omega_{2002}D_{z,South}$				−0.661***	−0.400**	−0.822***
				(0.180)	(0.162)	(0.204)
$\theta_{ant}\omega_{2003}D_{z,South}$				−0.733***	−0.439**	−0.961***
				(0.240)	(0.217)	(0.266)
$\theta D_{z,South}$				−1.317***	−0.919***	−1.596***
				(0.202)	(0.187)	(0.229)
N (postal codes)	27,835 (146)	27,835 (146)	27,835 (146)	27,835 (146)	27,835 (146)	27,835 (146)
Within R^2	0.869	0.869	0.869	0.870	0.870	0.870

All results are based on postal code fixed effects regressions. Standard errors are clustered at the postal code level (in parentheses). Year fixed effects, month fixed effects and hedonic controls for house characteristics are included as well as postal code specific linear time trends.
*/**/***denote significance at the 10%, 5% and 1% level.

Zeeland and there is plenty of vacant land available for construction (CPB & PBL, 2015). Third, our finding that the effect of the tunnel capitalizes into house prices within 2 years corroborates the idea that supply adjustment plays at best a minor role (since a short period like this is probably insufficient to develop and construct new housing on a large scale).

The type of housing supply as measured by the share of recreational houses (which are less sensitive to changes in accessibility) also does not seem to drive our results, i.e. recreational housing is not more common in the southern than in the northern region. A final explanation for the regionally different impact may be that the southern region has a higher housing vacancy rate than the northern region. This would potentially facilitate housing market adjustment to demand shocks in the southern region through other channels than prices. We were not able to test this.

4.2. Sensitivity analyses

In this subsection, we explore the robustness of our main findings to alternative distance decay functions, de-trended house prices and different sample periods.

Table 7. Robustness to an inverse distance weighted accessibility measure

	Inverse $\alpha = 0.75$	Inverse $\alpha = 1$	Inverse $\alpha = 2$	Inverse $\alpha = 0.75$ with regional interaction	Inverse $\alpha = 1$ with regional interaction	Inverse $\alpha = 2$ with regional interaction
$\theta_{ant}\omega_{2000}$	0.307	0.295	0.424	0.653**	0.626**	0.931**
	(0.195)	(0.201)	(0.291)	(0.244)	(0.252)	(0.392)
$\theta_{ant}\omega_{2001}$	0.242	0.237	0.486*	0.767***	0.740***	1.133***
	(0.202)	(0.204)	(0.247)	(0.215)	(0.222)	(0.318)
$\theta_{ant}\omega_{2002}$	0.601**	0.621**	0.672	1.146***	1.151***	1.491***
	(0.263)	(0.272)	(0.446)	(0.267)	(0.271)	(0.378)
$\theta_{ant}\omega_{2003}$	0.730**	0.757**	0.546	1.424***	1.427***	1.384**
	(0.325)	(0.336)	(0.557)	(0.329)	(0.330)	(0.536)
θ	0.958***	0.989***	0.923	1.908**	1.915***	2.052***
	(0.341)	(0.351)	(0.561)	(0.326)	(0.327)	(0.533)
$\theta_{ant}\omega_{2000}D_{z,South}$				−0.599**	−0.593***	−1.137***
				(0.199)	(0.204)	(0.360)
$\theta_{ant}\omega_{2001}D_{z,South}$				−0.897***	−0.891***	−1.606***
				(0.175)	(0.180)	(0.341)
$\theta_{ant}\omega_{2002}D_{z,South}$				−0.940***	−0.947***	−1.973***
				(0.222)	(0.224)	(0.395)
$\theta_{ant}\omega_{2003}D_{z,South}$				−1.029***	−1.028***	−1.911***
				(0.303)	(0.304)	(0.485)
$\theta D_{z,South}$				−1.671***	−1.682***	−2.842***
				(0.269)	(0.268)	(0.423)
N (postal codes)	27,835 (146)	27,835 (146)	27,835 (146)	27,835 (146)	27,835 (146)	27,835 (146)
Within R^2	0.869	0.869	0.869	0.870	0.870	0.869

All results are based on postal code fixed effects regressions. Standard errors are clustered at the postal code level (in parentheses). Year fixed effects, month fixed effects and hedonic controls for house characteristics are included as well as postal code specific linear time trends.
*/**/***denote significance at the 10%, 5% and 1% level.

4.2.1. Alternative distance decay functions

Our main accessibility indicator is based on the estimates for people's willingness to commute for τ minutes in the Netherlands (see De Groot et al., 2015). In what follows, we test the robustness of our results with respect to other accessibility measures. Table 6 shows the results of Equation (2) when the cutoff value of the distance decay function is changed from a travel time of 90 minutes to 60 and 120 minutes. Our baseline results (using the 90-minute cutoff value) are robust to different cutoff values, although the effect size and significance tend to decrease in the estimations without regional interaction. The estimates including regional interaction yield very similar results, i.e. the accessibility effect is dominated by the northern region.

We also employ an inverse travel time weighted accessibility measure: $F(\tau_{zdt}) = 1/\tau_{zdt}^{\alpha}$, where α is a power parameter (e.g. Levkovich et al., 2016). Inverse distance decay functions attribute more weight to economic mass close to postal code z (left tail) than our Gaussian function does, especially for high values of α (see Figure 4). The right tail of the inverse function tends to be fatter, particularly for low values of α. We distinguish three values of α: 0.75, 1 and 2. Table 7 shows that the estimated

Table 8. Robustness with respect to a tricube weighted accessibility measure

	Tricube Cutoff 60	Tricube Cutoff 90	Tricube Cutoff 120	Tricube Cutoff 60 with regional interaction	Tricube Cutoff 90 with regional interaction	Tricube Cutoff 120 with regional interaction
$\theta_{ant}\omega_{2000}$	−0.131	0.016	0.179	−0.016	−0.009	0.389
	(0.099)	(0.141)	(0.171)	(0.232	(0.135)	(0.216)
$\theta_{ant}\omega_{2001}$	−0.210*	0.046	0.142	0.013	0.058	0.497**
	(0.110)	(0.1483)	(0.185)	(0.231)	(0.145)	(0.206)
$\theta_{ant}\omega_{2002}$	0.118	0.405**	0.561**	0.527**	0.422***	0.948***
	(0.160)	(0.165)	(0.235)	(0.246)	(0.162)	(0.234)
$\theta_{ant}\omega_{2003}$	0.179	0.627***	0.741**	0.793***	0.578***	1.240***
	(0.215)	(0.187)	(0.281)	(0.300)	(0.192)	(0.269)
θ	0.272	0.824***	0.965***	1.247***	0.879***	1.672***
	(0.213)	(0.179)	(0.294)	(0.267)	(0.183)	(0.260)
$\theta_{ant}\omega_{2000}D_{z,South}$				−0.166	−0.314**	−0.389**
				(0.196)	(0.128)	(0.160)
$\theta_{ant}\omega_{2001}D_{z,South}$				−0.296	−0.491***	−0.631***
				(0.181)	(0.119)	(0.148)
$\theta_{ant}\omega_{2002}D_{z,South}$				−0.520***	−0.417**	−0.750***
				(0.185)	(0.171)	(0.186)
$\theta_{ant}\omega_{2003}D_{z,South}$				−0.643**	−0.334	−0.788***
				(0.245)	(0.219)	(0.253)
$\theta D_{z,South}$				−1.239***	−0.807***	−1.428***
				(0.198)	(0.199)	(0.220)
N (postal codes)	27,835 (146)	27,835 (146)	27,835 (146)	27,835 (146)	27,835 (146)	27,835 (146)
Within R^2	0.869	0.869	0.869	0.870	0.870	0.870

All results are based on postal code fixed effects regressions. Standard errors are clustered at the postal code level (in parentheses). Year fixed effects, month fixed effects and hedonic controls for house characteristics are included as well as postal code specific linear time trends.
*/**/***denote significance at the 10%, 5% and 1% level.

accessibility elasticities using different power values do not differ much from one another. Moreover, the results are again very similar to our main results.

Finally, a tricube weighting function with cutoff value ψ (60, 90 and 120 minutes travel time) is used to construct an alternative accessibility measure: $F(\tau_{zdt}) = \max\left[\left(1-\left(\frac{\tau_{zdt}}{\psi}\right)^3\right)^3, 0\right]$ (see Koster, 2013). In comparison with our main estimation, it attributes a relatively low weight to the tails of the distribution, especially for high levels of ψ (see Figure 4). Overall, this alternative accessibility measure yields outcomes that are similar to those from our main specification (see Table 8).

4.2.2. De-trended house prices

As mentioned before, the postal code specific linear time trends and the accessibility increase are negatively correlated (−0.30). Hence, the change in accessibility would be endogenous if we do not include the linear time trends. Another option to control for this correlation is by constructing a counterfactual house price in which the endogenous

Table 9. Robustness to de-trended house prices

	Equation (2) with de-trended house prices	Delayed response with de-trended house prices	Equation (2) with regional interaction and de-trended house prices
$\theta_{ant}\omega_{2000}$	0.250*	0.250*	0.228
	(0.136)	(0.136)	(0.171)
$\theta_{ant}\omega_{2001}$	0.296**	0.297**	0.301*
	(0.141)	(0.141)	(0.167)
$\theta_{ant}\omega_{2002}$	0.794***	0.793***	0.720**
	(0.245)	(0.245)	(0.283)
$\theta_{ant}\omega_{2003}$	0.960***	0.950***	0.802**
	(0.280)	(0.242)	(0.366)
θ	1.147***	1.131***	0.927**
	(0.318)	(0.237)	(0.431)
$\theta_{del}\omega_{2008-2013}$		0.036	
		(0.330)	
$\theta_{ant}\omega_{2000}D_{z,South}$			0.085
			(0.121)
$\theta_{ant}\omega_{2001}D_{z,South}$			0.035
			(0.111)
$\theta_{ant}\omega_{2002}D_{z,South}$			0.165
			(0.191)
$\theta_{ant}\omega_{2003}D_{z,South}$			0.271
			(0.280)
$\theta D_{z,South}$			0.437
			(0.329)
N (postal codes)	27,207 (126)	27,207 (126)	27,207 (126)
Within R^2	0.837	0.838	0.838

All results are based on postal code fixed effects regressions, after de-trending the house prices at the postal code level. Standard errors are clustered at the postal code level (in parentheses). Year fixed effects, month fixed effects and hedonic controls for house characteristics are included.
*/**/***denote significance at the 10%, 5% and 1% level.

house price trend has been eliminated. To create this counterfactual, we extrapolate the (linear) 1995–2001 house price trend per postal code to the year 2013 and subtract this trend from the house prices, before (re)estimating the regressions.[24] This de-trending exercise successfully eliminates the hazardous correlation: the correlation between the remaining house price trend and accessibility is 0.02 (p-value 0.79).

Moreover, the results from this exercise resemble our main findings (see Table 9). The estimates for Equation (2) with de-trended house prices tend to be higher (1.15 vs. 0.79), but the difference is insignificant. Again, we find no evidence for delayed response (see column 2). The regional interaction estimation turns out somewhat different than before: the southern region no longer profits less from the tunnel than the northern region (see column 3).

24 The pre-tunnel years, 2002 and 2003 (until 14 March), are excluded due to the anticipation effects. We also drop 20 postal codes since the sample size in these postal codes is too small to accurately estimate the 1995–2001 house price trend.

Table 10. Robustness to different sample periods

	1995–2007	1998–2013	1998–2007	1995–2007 with regional interaction	1998–2013 with regional interaction	1998–2007 with regional interaction
$\theta_{ant}\omega_{2000}$	0.156	0.116	0.142	0.139	0.134	0.086
	(0.131)	(0.133)	(0.128)	(0.202)	(0.189)	(0.190)
$\theta_{ant}\omega_{2001}$	0.166	0.037	0.102	0.190	0.196	0.119
	(0.132)	(0.139)	(0.136)	(0.189)	(0.189)	(0.194)
$\theta_{ant}\omega_{2002}$	0.644***	0.453***	0.525***	0.619***	0.658***	0.519**
	(0.153)	(0.173)	(0.160)	(0.193)	(0.194)	(0.203)
$\theta_{ant}\omega_{2003}$	0.951***	0.622***	0.776***	0.933***	0.969***	0.817***
	(0.178)	(0.231)	(0.196)	(0.237)	(0.242)	(0.254)
θ	1.028***	0.782***	0.796***	1.157***	1.389***	1.026***
	(0.177)	(0.237)	(0.200)	(0.238)	(0.224)	(0.262)
$\theta_{ant}\omega_{2000}D_{z,South}$				0.016	−0.065	−0.079
				(0.162)	(0.155)	(0.158)
$\theta_{ant}\omega_{2001}D_{z,South}$				−0.056	−0.302	−0.051
				(0.157)	(0.145)	(0.165)
$\theta_{ant}\omega_{2002}D_{z,South}$				0.021	−0.388**	−0.027
				(0.1682)	(0.160)	(0.180)
$\theta_{ant}\omega_{2003}D_{z,South}$				0.093	−0.474**	−0.006
				(0.222)	(0.219)	(0.239)
$\theta D_{z,South}$				−0.259	−1.122***	−0.462*
				(0.222)	(0.186)	(0.262)
N (postal codes)	27,835 (146)	25,087 (146)	17,052 (146)	19,800 (146)	25,087 (146)	17,052 (146)
Within R^2	0.869	0.850	0.864	0.881	0.851	0.864

All results are based on postal code fixed effects regressions. Standard errors are clustered at the postal code level (in parentheses). Year fixed effects, month fixed effects and hedonic controls for house characteristics are included as well as postal code specific linear time trends.
*/**/***denote significance at the 10%, 5% and 1% level.

4.2.3. Different sample periods

To test whether the available data period affects our main findings, we restrict our sample to specific subperiods. Table 10 reports the results for this robustness check using Equation (2). Without regional interaction, the impact of accessibility on house prices turns out to be rather insensitive to the sample period of interest. That is, the coefficients for the different subperiods by and large fluctuate within the confidence intervals of the main specification (1995–2013). However, if we include a regional interaction term, the distribution of the treatment effect across regions is less robust. Especially when the crisis years from 2008 onwards are deleted, the northern (southern) region tends to profit less (more) from the opening of the tunnel. Nevertheless, the accessibility effect for the southern region always remains smaller than for the northern region though not always significant.

5. Conclusion

This article studies the impact of accessibility on house prices based on a quasi-experiment in the Netherlands: the Westerscheldetunnel. We exploit the novel opportunity that the opening of the tunnel and the simultaneous abolishment of the ferry services caused a substantial shift in accessibility for people and firms in the connected regions, positively as well as negatively. Large variation in accessibility is a necessary condition to accurately measure the effect of accessibility on house prices. Nowadays, it is hard to find new transport infrastructure that generates such a shift in accessibility since most Western countries already have a (very) dense transport network.

Our results indicate that the accessibility elasticity of house prices is equal to 0.8. Approximately half of the effect already materializes more than 1 year before the opening of the tunnel. We do not find evidence for delayed response, i.e. all accessibility benefits due to the tunnel were absorbed in house prices in the year the tunnel was opened. Moreover, our findings suggest that the impact of accessibility differs substantially across regions. The northern region profited most from accessibility gains, whereas the southern region did not respond at all to a change in accessibility in most specifications. Our analyses of underlying mechanisms show that heterogeneity among residents is the most plausible explanation for the regionally different capitalization pattern.

Several limitations pertain to the quasi-experiment that we study. For instance, as is the case with any investment in transport infrastructure, the decision where to exactly locate the Westerscheldetunnel may not have been entirely random. Arguably, the tunnel had to connect properly to the existing transport network. External developments in the area of research also could have affected the estimated relationship between house prices and accessibility. Nevertheless, the tunnel can be qualified as a rather exogenous event compared to the bulk of transport infrastructure investments since the middle of the estuary was the only location that allowed the abolishment of both the eastern and western ferry service (which was a necessary condition to finance the construction of the tunnel). Moreover, our identification strategy—that aims to correct for possible bias as a result of time (in)variant sources—and the existence of natural borders in the Dutch province of Zeeland help to limit the influence of potentially confounding factors. All in all, we are confident that the quasi-experiment of the Westerscheldetunnel is informative about the causal effect of accessibility on house prices.

Yet, the evidence in this study also reveals that the precise impact of accessibility is subtle: anticipation might play an important role and effects may differ substantially across regions. A next step for future research would, therefore, be to examine quasi-experiments like in this study, preferably in a different context and on other economic outcomes such as employment, productivity or new establishments.

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Appendix

A. (Hedonic) control variables

This article uses several hedonic house characteristics from the NVM dataset and other control variables, such as postal code fixed effects and postal code specific linear time trends. Table A1 shows the mean values of these variables and their effect on house prices, following Equation (2).

Table A1. Descriptive statistics of (hedonic) control variables and their effect on house prices

Variable	Mean value	Effect on log house prices
Lot size in m^2 (log)	5.551	0.188*** (0.006)
Living area in m^2 (log)	4.759	0.568*** (0.017)
Average floor height in m (log) (volume in m^3 divided by living area in m^2)	1.119	0.428*** (0.020)
Maintenance status		
Bad	0.004	(reference)
Bad—mediocre	0.002	0.228*** (0.065)
Mediocre	0.019	0.186*** (0.033)
Mediocre—reasonable	0.005	0.270*** (0.038)
Reasonable	0.112	0.331*** (0.032)
Reasonable—good	0.040	0.397*** (0.034)
Good	0.731	0.493*** (0.033)
Good—excellent	0.010	0.559*** (0.035)
Excellent	0.079	0.578*** (0.034)
Housing type		
Row house	0.360	−0.196*** (0.014)
Semi-detached house	0.023	−0.135*** (0.013)
Corner house	0.170	−0.185*** (0.012)
Duplex house	0.181	−0.097*** (0.010)
Detached house	0.267	(reference)
Parking lot		
No parking place	0.556	(reference)
Parking place on the street	0.016	0.021 (0.015)
Carport	0.020	0.052*** (0.010)
Single garage	0.366	0.080*** (0.006)
Carport and garage	0.008	0.084*** (0.012)
At least a double garage	0.034	0.097*** (0.010)
Period of construction		
Before 1906	0.087	−0.111*** (0.016)
Between 1906 and 1930	0.136	−0.168*** (0.015)
Between 1931 and 1944	0.065	−0.171*** (0.017)
Between 1945 and 1959	0.105	−0.169*** (0.012)
Between 1960 and 1970	0.156	−0.152*** (0.010)
Between 1971 and 1980	0.212	−0.115*** (0.013)
Between 1981 and 1990	0.101	−0.055*** (0.013)
Between 1991 and 2000	0.110	(reference)
2000 and later	0.026	0.014 (0.012)

(continued)

Table A1. Continued

Variable	Mean value	Effect on log house prices
Central heating system		
No	0.126	(reference)
Yes	0.874	0.099*** (0.007)
Year of transaction		
1995	0.026	(reference)
1996	0.034	0.080*** (0.010)
1997	0.039	0.123*** (0.011)
1998	0.041	0.181*** (0.012)
1999	0.049	0.306*** (0.014)
2000	0.054	0.406*** (0.017)
2001	0.065	0.497*** (0.017)
2002	0.064	0.573*** (0.016)
2003	0.062	0.625*** (0.016)
2004	0.063	0.667*** (0.015)
2005	0.072	0.716*** (0.014)
2006	0.072	0.742*** (0.012)
2007	0.070	0.762*** (0.011)
2008	0.058	0.775*** (0.011)
2009	0.044	0.728*** (0.010)
2010	0.044	0.728*** (0.011)
2011	0.043	0.709*** (0.012)
2012	0.050	0.656*** (0.014)
2013	0.050	0.624*** (0.016)
Month of transaction		
January	0.070	(reference)
February	0.079	0.006 (0.006)
March	0.086	0.013** (0.006)
April	0.084	0.022*** (0.006)
May	0.085	0.029*** (0.005)
June	0.086	0.022*** (0.005)
July	0.088	0.033*** (0.006)
August	0.083	0.030*** (0.005)
September	0.085	0.036*** (0.006)
October	0.093	0.038*** (0.005)
November	0.084	0.037*** (0.006)
December	0.077	0.040*** (0.005)
Postal code specific linear time trends (146)		−0.024*** to 0.031***, [a]
Constant		−2.775 (3.312)
N (postal codes)		27,835 (146)
Within R^2		0.869

Standard errors are clustered at the postal code level (in parentheses).

[a]This effect varies over postal codes; the table shows the minimum and maximum value.

*/**/***denote significance at the 10%, 5% and 1% level.

B. Housing data selection procedure

The initial dataset has been cleared to remove incomplete observations. One may think of houses with undisclosed selling price or with unknown lot size. In addition, we have deleted apartments as well as a few houses that have been sold more than five times during the period between 1985 and 2013. Table B1 summarizes our data selection procedure in sequential order.

Table B1. Housing data selection procedure and number of observations

Selection criteria[a]	Number of observations
1. Initial dataset (1985–2013)	38,948
2. Remove cases with zero living area	
3. Remove cases with zero lot size	
4. Remove cases with lot size outside 0.1 and 99.9 percentiles	
5. Remove cases with average floor height outside 0.1 and 99.9 percentiles	
6. Remove cases with unknown transaction price	
7. Remove cases with transaction price outside 0.5 and 99.5 percentiles	
8. Remove cases with no information on parking facilities	
9. Remove data on apartments	
10. Remove cases with unknown dwelling type	
11. Remove cases with unknown maintenance status	
12. Remove cases with no information on the presence of central heating	
13. Remove cases with unknown year of construction	
14. Remove dwellings that have been sold more than 5 times	
15. Remove postal codes with less than 15 observations in 1985–2013	31,573
16. Remove years before 1995 (due to representativeness concerns)	27,835

[a]Some numbers in the table are missing due to confidentiality of the NVM dataset. These numbers can be obtained upon request.

C. Sensitivity to minimum number of house transactions per postal code

Table C1 shows that adjusting the minimum number of house transactions yields similar results as the main specification. Hence, the current minimum of 13 transactions per postal code seems to be sufficient to run the analyses accurately. The estimations are even more robust if we add a regional interaction dummy (see Table C2).

Table C1. Effect of accessibility on house prices with different minimum number of house transactions per postal code, Equation (2)

	Baseline estimation (13 transactions)	50 transactions	100 transactions
$\theta_{ant}\omega_{2000}$	0.033 (0.161)	−0.046 (0.169)	−0.077 (0.142)
$\theta_{ant}\omega_{2001}$	−0.031 (0.173)	−0.029 (0.180)	−0.083 (0.175)
$\theta_{ant}\omega_{2002}$	0.427* (0.218)	0.447** (0.225)	0.354 (0.240)
$\theta_{ant}\omega_{2003}$	0.599** (0.271)	0.574** (0.282)	0.392 (0.308)
θ	0.790*** (0.277)	0.788*** (0.290)	0.642** (0.318)
N (postal codes)	27,835 (146)	26,456 (105)	24,272 (75)
Within R^2	0.869	0.869	0.871

All results are based on postal code fixed effects regressions. Standard errors are clustered at the postal code level (in parentheses). Year fixed effects, month fixed effects and hedonic controls for house characteristics are included as well as postal code specific linear time trends.
*/**/***denote significance at the 10%, 5% and 1% level.

Table C2. Effect of accessibility on house prices with different minimum number of house transactions per postal code, Equation (2) with regional interaction

	Baseline estimation (13 transactions)	50 transactions	100 transactions
$\theta_{ant}\omega_{2000}$	0.219 (0.230)	0.178 (0.244)	0.043 (0.211)
$\theta_{ant}\omega_{2001}$	0.279 (0.221)	0.289 (0.230)	0.235 (0.233)
$\theta_{ant}\omega_{2002}$	0.781*** (0.236)	0.815*** (0.242)	0.717*** (0.265)
$\theta_{ant}\omega_{2003}$	1.077*** (0.273)	1.056*** (0.285)	0.901*** (0.318)
θ	1.497*** (0.257)	1.524*** (0.270)	1.451*** (0.310)
$\theta_{ant}\omega_{2000}D_{z,South}$	−0.363** (0.179)	−0.344* (0.185)	−0.212 (0.162)
$\theta_{ant}\omega_{2001}D_{z,South}$	−0.578*** (0.163)	−0.568*** (0.166)	−0.490*** (0.164)
$\theta_{ant}\omega_{2002}D_{z,South}$	−0.661*** (0.180)	−0.658*** (0.180)	−0.560*** (0.177)
$\theta_{ant}\omega_{2003}D_{z,South}$	−0.733*** (0.240)	−0.691*** (0.241)	−0.578** (0.244)
$\theta D_{z,South}$	−1.317*** (0.202)	−1.315*** (0.204)	−1.259*** (0.212)
N (postal codes)	27,835 (146)	26,456 (105)	24,272 (75)
Within R^2	0.870	0.869	0.872

All results are based on postal code fixed effects regressions. Standard errors are clustered at the postal code level (in parentheses). Year fixed effects, month fixed effects and hedonic controls for house characteristics are included as well as postal code specific linear time trends.
*/**/***denote significance at the 10%, 5% and 1% level.

D. Anticipation effects per quarter

Table D1 reports the results for Equation (2) if we employ anticipation variables for the first quarter of 2000 until the first quarter of 2003 (instead of using yearly anticipation variables). Anticipation turns significant in the third quarter of 2002 and increases towards the last quarter of 2002. Compared to the previous quarter, there is no additional anticipation effect in the first quarter of 2003.

Table D1. Effect of accessibility on house prices with anticipation per quarter, Equation (2)

	Effect on log house prices
$\theta_{ant}\omega_{2000,q1}$	−0.294* (0.176)
$\theta_{ant}\omega_{2000,q2}$	0.022 (0.156)
$\theta_{ant}\omega_{2000,q3}$	0.096 (0.169)
$\theta_{ant}\omega_{2000,q4}$	0.222 (0.186)
$\theta_{ant}\omega_{2001,q1}$	−0.176 (0.177)
$\theta_{ant}\omega_{2001,q2}$	−0.090 (0.176)
$\theta_{ant}\omega_{2001,q3}$	0.027 (0.202)
$\theta_{ant}\omega_{2001,q4}$	0.131 (0.183)
$\theta_{ant}\omega_{2002,q1}$	0.214 (0.227)
$\theta_{ant}\omega_{2002,q2}$	0.350 (0.220)
$\theta_{ant}\omega_{2002,q3}$	0.479** (0.230)
$\theta_{ant}\omega_{2002,q4}$	0.606** (0.243)
$\theta_{ant}\omega_{2003,q1}$	0.568** (0.270)
θ	0.793*** (0.277)
N (postal codes)	27,835 (146)
Within R^2	0.869

All results are based on postal code fixed effects regressions. Standard errors are clustered at the postal code level (in parentheses). Year fixed effects, month fixed effects and hedonic controls for house characteristics are included as well as postal code specific linear time trends.
*/**/***denote significance at the 10%, 5% and 1% level.

E. Results with an accessibility measure based on population

All results in this article are based on the accessibility of jobs. Table E1 shows that similar results for Table 3 are obtained once we calculate accessibility on the basis of population rather than jobs.

Table E1. Effect of accessibility on house prices using an accessibility measure based on population

	Equation (1)	Equation (2)	Equation (3)	Equation (4)
$\theta_{ant}\omega_{2000}$		0.058		0.115
		(0.167)		(0.158)
$\theta_{ant}\omega_{2001}$		0.014		0.092
		(0.174)		(0.161)
$\theta_{ant}\omega_{2002}$		0.494**		0.594***
		(0.206)		(0.190)
$\theta_{ant}\omega_{2003}$		0.700***		0.834***
		(0.246)		(0.228)
θ	0.539***	0.907***	0.508***	1.050***
	(0.116)	(0.247)	(0.102)	(0.224)
$\theta_{del}\omega_{2008-2013}$			−0.106	0.163
			(0.134)	(0.107)
N (postal codes)	27,835 (146)	27,835 (146)	27,835 (146)	27,835 (146)
Within R^2	0.869	0.869	0.869	0.869

All results are based on postal code fixed effects regressions. Standard errors are clustered at the postal code level (in parentheses). Year fixed effects, month fixed effects and hedonic controls for house characteristics are included as well as postal code specific linear time trends.
*/**/***denote significance at the 10%, 5% and 1% level.

F. Results with a finer-grained postal code fixed effects interaction

The last column of Table 5 shows the results of a regression with a postal code fixed effects interaction term using a dummy variable that equals 1 if the postal code's fixed effect was larger than the median value, and 0 otherwise. Table F1 presents the results for a finer-grained division of postal code fixed effects into tertiles and quartiles.

Table F1. Effect of accessibility on house prices with a finer-grained postal code fixed effects interaction

	With postal code fixed effects interaction (tertiles)	With postal code fixed effects interaction (quartiles)
$\theta_{ant}\omega_{2000}$	−0.559*	−0.663
	(0.324)	(0.547)
$\theta_{ant}\omega_{2001}$	−0.797**	−0.845*
	(0.313)	(0.470)
$\theta_{ant}\omega_{2002}$	−0.355	−0.361
	(0.343)	(0.475)
$\theta_{ant}\omega_{2003}$	−0.239	0.145
	(0.523)	(0.569)
θ	−0.589	−0.111
	(0.500)	(0.521)
$\theta_{ant}\omega_{2000}Q_2$	0.625*	0.726
	(0.321)	(0.532)
$\theta_{ant}\omega_{2001}Q_2$	0.755***	0.796*
	(0.270)	(0.435)
$\theta_{ant}\omega_{2002}Q_2$	0.802***	0.726*
	(0.289)	(0.430)
$\theta_{ant}\omega_{2003}Q_2$	0.893**	0.364
	(0.451)	(0.524)
θQ_2	1.247***	0.606
	(0.440)	(0.476)
$\theta_{ant}\omega_{2000}Q_3$	0.410	0.409
	(0.332)	(0.524)
$\theta_{ant}\omega_{2001}Q_3$	0.721**	0.609
	(0.293)	(0.424)
$\theta_{ant}\omega_{2002}Q_3$	0.639**	0.754*
	(0.280)	(0.410)
$\theta_{ant}\omega_{2003}Q_3$	0.655	0.685
	(0.448)	(0.524)
θQ_3	1.568***	1.186***
	(0.392)	(0.445)
$\theta_{ant}\omega_{2000}Q_4$		0.828
		(0.520)
$\theta_{ant}\omega_{2001}Q_4$		1.167***
		(0.430)
$\theta_{ant}\omega_{2002}Q_4$		0.945**
		(0.407)
$\theta_{ant}\omega_{2003}Q_4$		0.609
		(0.522)
θQ_4		1.417***
		(436)
N (postal codes)	27,835 (146)	27,835 (146)
Within R^2	0.869	0.870

All results are based on postal code fixed effects regressions. Standard errors are clustered at the postal code level (in parentheses). Year fixed effects, month fixed effects and hedonic controls for house characteristics are included as well as postal code specific linear time trends.
*/**/***denote significance at the 10%, 5% and 1% level.