Highway expansions and local economic activity

Work in progress

Ioulia V. Ossokina, Jos van Ommeren and Henk van Mourik

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Abstract – In many countries, highway construction occurs nowadays mainly through expansions – adding new lanes to existing corridors – rather than through construction of new greenfield highways. The economic effects of expansions have hardly been studied. We estimate the effect of these expansions on employment at a detailed spatial level exploiting information about all highway expansions during a period of 25 years for the Netherlands. Our identification strategy uses that the precise construction year of expansions is highly random. We demonstrate that highway expansions rather quickly – within 3 to 5 years after the expansion – induce a redistribution of local employment. Adding 10 km lane increases employment within 5 km of the highway by 3 percent, but reduces employment further away (between 5 to 10 km). Our results contribute to the ongoing public discussion about the local costs and benefits of highway investments.

Keywords: highways, expansions, employment

JEL Codes: R3, R33, R4, R42, H4

1 Introduction

In many countries, plans for highway construction and expansion meet fierce local protests. This has led to altering, postponing and sometimes even stopping of proposed highway investments, see Brinkman and Lin (2017) and Ossokina and Verweij (2015). While neighbourhood advocates claim considerable negative externalities from pollution, noise and spatial barriers induced by motorists using these highways, they are usually sceptical about possible positive effects on local economic growth.

Highway construction occurs nowadays largely through expansions – adding new lanes to existing corridors – rather than through construction of new greenfield highways. In the Netherlands for instance, only few new greenfield highways were built after 1990, whereas highway lane kilometres increased during this period substantially (about 25%). Most developed countries show a similar pattern of slowing down new highway construction (e.g. Germany, France, Italy, UK, see Eurostat, 2015). While effects of greenfield highways have received a lot of attention in the literature since the seminal studies by Baum-Snow (2007; 2010), highway expansions have not been studied (likely because data about expansions are rare). This is a bit unfortunate, because arguably to inform policymakers of developed countries about the effect of policies currently considered, we need to know more about the economic effects of planned expansions in the near future.[1]

In the current paper, we are interested in effect of highways expansions on *local* economic activity (within 10 km), and will focus on relatively short run effects (less than 10 years), whereas the existing literature on greenfield highways focuses exclusively on regional economic activity in the long run (after 10 years) and tends to ignore local effects.

From a theoretical perspective, the economic effects of expansions are likely substantially different – e.g. in terms of their size, geography as well as time – from the effects of the construction of greenfield highways. First, expansions do not reduce the number of kilometres of highway between destinations and will therefore only have an effect on economic activity given the presence of (substantial) travel time delays on the existing highway. Hence, highway expansions will usually have no effect on travel times outside rush hours. In contrast, greenfield highways always reduce travel times because the number of highway kilometres between destinations is reduced. Hence, the time gain *per motorist* induced by expansions will likely be an order of magnitude smaller than that of greenfield highways. Second, when highways are expanded the flow of motorists per unit of time tends to be high (usually close to maximum capacity during rush hours), whereas for new highways analysed in the literature (especially those built during periods when car ownership was still relatively low) the flow of motorists tends to be low. This suggests that the immediate overall time gain for motorists due to expansions may easily exceed those of new highways.[2] Hence, in contrast to greenfield

[1] We will discuss data for the Netherlands about *planned* highways which indicate that highway *expansions* will continue to grow at the same rate as in previous decades, whereas there are practically no new highways planned.

[2] The implicit – but we believe reasonable – assumption we make here is that expansions only occur given the presence of highway congestion, so travel time savings are the main goal of the expansion.

highways, where most effects are expected in the long run, it is plausible that most of the effects of expansions occur rather immediately, i.e. in the short run.[3]

In the current paper, we estimate the effect of highway expansions (i.e. an increase in the number of highway km lanes) on changes in local employment (we measure employment at the zip code level) for the Netherlands between 1996 and 2013.[4] We distinguish between effects of highway expansions on employment in the close vicinity of a zip code (0 to 5 kilometre) and further away (5 to 10 kilometres). We employ data on the number of lanes for all highway segments which vary substantially over time during the study period. [5] The spatial and time variation in the number of lanes is used for identification. We estimate zip code fixed effects models of employment to control for unobserved time-invariant heterogeneity.[6] Note that this identification strategy differs from the identification strategy used in the greenfield highway literature, where it is common to use instrument variable techniques using historic instruments (e.g. Baum Snow, 2007).

Inclusion of zip code fixed effects does not control for time-varying heterogeneity which may be problematic, when time-varying location characteristics are correlated to highway expansions. For example, highways might be expanded at locations where employment is expected to grow (Duranton and Turner, 2012). We take several steps to account for this endogeneity issue.

First, we only select zip codes that faced a nearby highway expansion in their vicinity. This relaxes the identifying assumption for causal inference, as it only requires that, conditional on the highway having been expanded, the *exact timing* of the expansion is random (i.e, not correlated to employment growth). To justify this assumption we discuss the transportation investment planning for the Netherlands and explain that it involves several phases of consulting with several layers of government and local residents which lead to unpredictable – and therefore arguably random – adjustments to the exact timing of the expansion.[7]

Second, we note that changes in employment usually takes place on a larger spatial scale than that of a single zip code, i.e unobserved employment shocks are strongly spatially correlated. Hence, we include region-specific time trends, distinguishing between regions, which include on average only four zip codes.[8] Hence, identification comes from time-variation in

[3] It is plausible that long-run effects on economic activity of expansions are even smaller than short run effects of expansions on economic activity *for locations close to highway expansions*, because increased demand for highways travel by motorists who travel between destinations further away will locally increase congestion again towards its old level (Duranton and Turner, 2011).
[4] A four digit zip code is small. In cities it covers around 1 square kilometre and contains about 2000 buildings (dwellings and commercial premises).
[5] To be more precise, we will use highway data starting five years before the study period (to estimate the effect of lags of highway expansions) as well as after the study period (to estimate the effect of leads of highway expansions, in order to estimate placebo effects). In the Netherlands, there are 260 highway segments with an average length of 8 kilometre.
[6] To account for the possibility of anticipation effects and/or slow employment adjustment, we estimate models with time lags and leads with a length of up to 5 years.
[7] We show that only a small proportion (about 10%) of the new lanes was opened in the planned year.
[8] In our data, we will define 347 regions.

employment for the zip codes located in the same region. Third, we demonstrate that controlling for the (changes in the) presence of the volume of commercial real estate buildings does not affect our estimates. This makes our identification strategy more convincing. Because newly constructed buildings have also been *planned* years in advance, robustness of the results to including the building stock supports our assumption that the precise year of highway openings can be considered random.

Our paper is connected to two streams of literature. The first one examines the impact of *greenfield* highways on urban development, and in particular the relocation of population and employment. Baum-Snow (2007, 2010) shows for the US in 1950-1990 that additional highways passing through central cities led to suburbanization of population and employment. Garcia Lopez et al. (2015) show that these effects have become smaller over the last decennia in Europe, likely because of the presence of a well-developed highway network. Levkovich et al. (2017) demonstrate that zoning policies influence how new highways affect population relocation in the Netherlands. Duranton and Turner (2012) show for the US that new highways caused increases in employment and population in metro areas in 1980-2000. Moeller and Zierer (2018) report substantial effects of new highways on employment in German regions. Compared to this literature, we estimate effects on a much more detailed spatial level. Furthermore, our innovation if to focus on highway *expansions* instead of greenfield highways.

Another relevant literature concerns studies of local costs and benefits of specific infrastructural investments. Gibbons and Machin (2005), Billings (2011), Levkovich et al. (2014), Ossokina and Verweij (2015) study the impact of concrete highway or railway investments on housing prices in the direct vicinity. We analyse local employment effects. Furthermore, while the literature focusses on case-studies, our estimates are based on a representative panel of highway expansions over 25 years.

Our results indicate that highway expansions lead to clustering and relocation of economic activity near highways. An additional 10 kilometre lane leads to a 3 percent increase in employment within the radius of 5 kilometres, but reduces employment further away (between 5 to 10 kilometres). These effects occur within 3 to 5 years after the opening and there is no anticipation. The results are robust to various robustness checks. The insights from our research are novel and important. This study is the first to consider the effects of expansions of existing highways, which are the focus of current and future policies.[9]

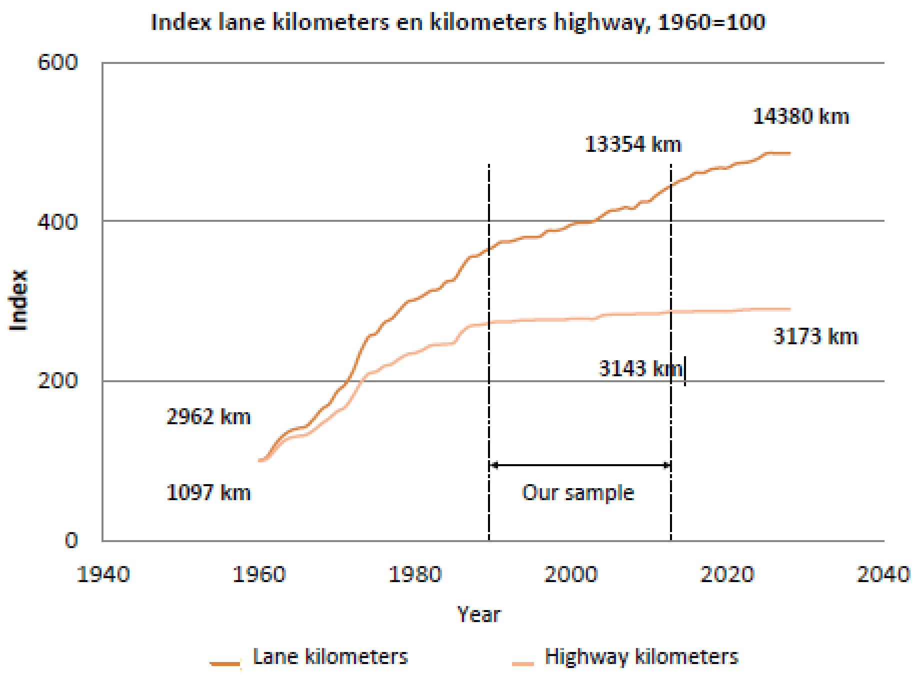
The structure of the paper is as follows. Section 2 discusses highway investments and in particular their planning process in the Netherlands, which is essential for our identification strategy. Section 3 discusses the empirical model and identification strategy. Section 4 deals with the data, Section 5 reports the estimation results, section 6 discusses sensitivity and robustness checks. Section 7 concludes.

[9] About 1% of Dutch GDP is spent on highway investment, mainly through adding new lanes and maintenance (Dutch Ministry of Transportation, 2016).

2 Highway investment, and its planning, for the Netherlands

In Figure 1, we show the total length of highways in the Netherlands, as well as the number of highway *lane* kilometres.[10] The highway length grew with a factor 3 between 1960 and 1990 but did not increase substantially since. In contrast, the number of *lanes* – and thus the number of lane kilometres - kept steadily rising. This figure also shows future plans: the number of highways lanes, but not the highway length, is expected to keep growing in the coming years. In Figure 2, it is shown that highway expansions took place on a large share of the highway network, providing a lot of spatial variation. In this paper we focus on the period from 1991 to 2016.

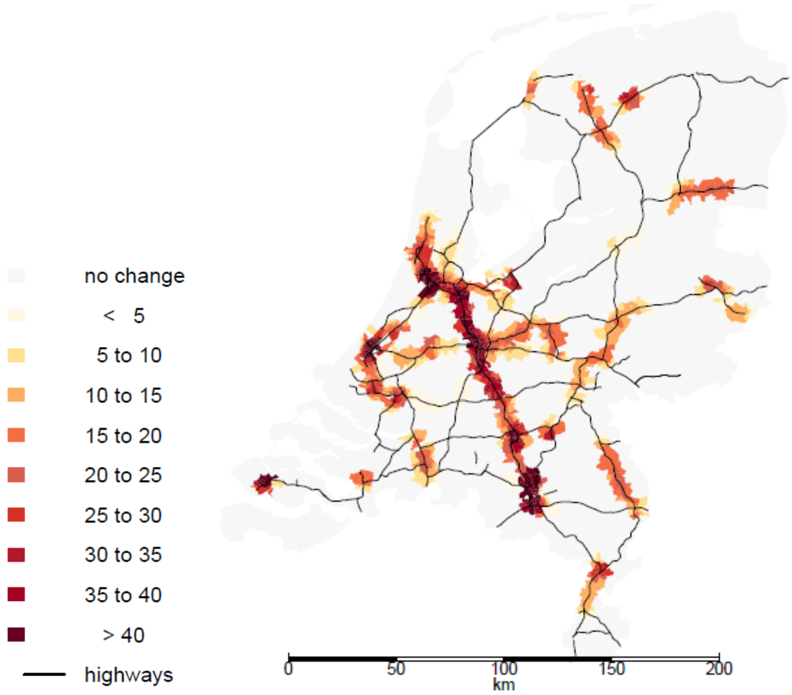
Figure 1. Kilometres highway and lane kilometres highway



The Netherlands has a high population density and has one of the most developed and densest highway networks the world (Eurostat and the World Economic Forum, 2015). It has a long history in spatial planning starting about 800 years ago. Similar to some other European countries (e.g. the UK), the political decision-making concerning highway investments is very involved: Usually many years pass between the time a new highway corridor or expansion is first *mentioned* in policy documents and the year of construction. Many rounds of consultations within different layers of the government, and with local residents, take place to ensure that most costs and benefits of new developments have been properly included.

[10] To calculate the number of lane kilometres, we multiply the length of highway segments by the number of lanes that the segment has, and sum this up for the whole network.

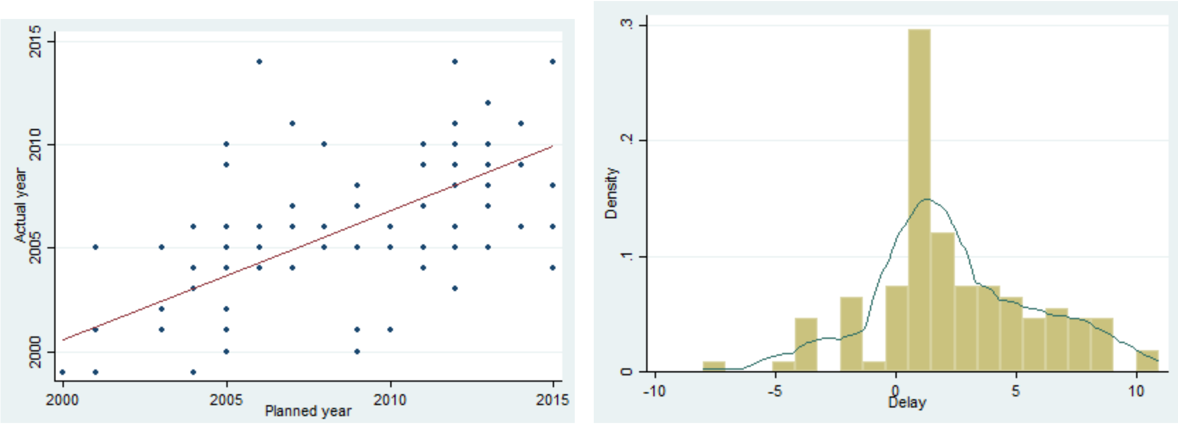
Figure 2. Number of added lane kilometres within the radius 0-5km, 1996-2013, by zip code



The decision-making process includes seven main steps.[11] Several of these steps take quite some time and allow for a possibility of appeal against the project. As a result, it is very difficult to predict the exact year in which a highway expansion will be opened. For a subsample of our data (highway expansions realised in 2000-2015), we know not only the realised year of opening, but also the planned opening year that was mentioned in the policy documents in the beginning of the decision-making. Figure 3 shows a scatter diagram of the realised opening year and the planned opening year. The R^2 of a linear relationship is only 0.47. It appears that only 10 percent of the expansions were realised in the planned year. 80% were delayed with up to 12 years and 10% were realised up to 7 years earlier than planned.

[11] (1) An announcement of new highway investment appears in the yearly Investment Program Infrastructure and Transport. (2) Initial research is performed, in which the project is examined for its desirability (no money available yet). (3) An Initial Memorandum of Announcement is published, under the responsibility of the minister of Housing, Physical Planning & the Environment, and the minister of Transport, Public Works & Water Management. The Memorandum describes the need for and desirability of the investment. Local governments, interest groups and concerned citizens have the right to react to this announcement. (4) The Memorandum is worked out further, in consultation with involved parties. A general Environmental Impact Report is produced, discussing different alternatives to the project. (5) A concrete Draft Alignment Decision is developed, that determines the precise location of the project. Public consultation takes place: the lower governments, special interest groups and private citizens are given the opportunity to offer input. (6) The Alignment Decision is taken and publicly announced, funding is guaranteed. It is possible to appeal against this decision with the Council of State. (7) The project is carried out.

Figure 3. Highway expansions 2000-2015: planned and actual year of the opening (left), distribution of delays (right)



3 Empirical model and identification

Our empirical model has the following form. Our dependent variable is the logarithm of employment *L* in a zip code *i* located in region *j* in time *t.* It makes sense that there is a slight delay in the effect of highway expansions on employment. Our focus is on the effect of the highway lane length in the vicinity of this zip code three years before, H_{ijt-3} (but we will also consider other time lags; we will experiment with different lags and leads using H_{ijt},). We distinguish between the effects nearby (captured by the dummy $D_{<5km}$, less than 5 km radius of *i*) and further away (dummy D_{5-10km}, 5 to 10 km radius of *i*). We control for year fixed effects, θ_t, zip code fixed effects, γ_i and region-specific time trends $\tau_j t_t$ (where a region contains several zip codes).

We use two types of specifications, which only differ in terms of one control variable:

(1a) $$\log L_{ijt} = \beta_1 H_{ijt-3} D_{<5km} + \beta_2 H_{ijt-3} D_{5-10km} + \alpha \log K_{ijt} + \gamma_i + \theta_t + \tau_j t_t + \varepsilon_{ijt,}$$

(1b) $$\log L_{ijt} = \beta_1 H_{ijt-3} D_{<5km} + \beta_2 H_{ijt-3} D_{5-10km} + \gamma_i + \theta_t + \tau_j t_t + \varepsilon_{ijt} ,$$

where ε_{ijt} is the error term.

In specification (*1a*), we also control for the size of commercial real estate in *i*, K_{ijt}. The underlying idea here is that by controlling for (changes in) commercial real estate buildings, essentially we control for planned shocks to employment. In specification (*1b*), we do not control for this variable, because it is not a priori clear whether controlling for this variable is preferred. To see this, noted that under the assumption that the construction of new commercial real estate is *not* the result of highway expansions, the first specification is preferred. Given the assumption that this construction *is* the result of highway expansions, the second specification is preferred because this control variable is a so-called 'bad cop': it picks up the variation we aim to explain (see Angrist and Pischke, 2009). It is plausible that the first specification provides an overestimate of the causal effect of highway expansions, whereas the second specification provides and underestimate of the causal effect in case these assumptions do not strictly hold. We will see that both specifications give almost identical results, which provides more confidence in the estimation strategy.[12]

The above equations generate consistent estimates of the effects of H_{ijt-3} under the assumption that H_{ijt-3} is exogenous, i.e. not correlated with ε_{ijt} . However, there might be reasons why this assumption does not hold and why areas which tend to grow are more likely to receive highway investments. We address this issue by restricting the analysis to the zip codes that during the study period faced a highway expansion. Hence, the identification assumption justifying our approach is that the exact timing of an expansion is assumed to be random, conditional on the presumption that governments may expand highways close to areas with higher employment growth. The planning process of highway investments discussed in the previous section provides support to this assumption. Note that there are also areas which receive more than one highway expansion during the period of observation. This may be problematic because it may be that areas which tend to grow faster are more likely to receive several expansions. To take this into account, we will mainly focus on a restricted dataset of locations which receive exactly one highway expansion (but in a sensitivity analysis we will show that the latter selection does not change the results).

We also estimate specifications with different lags and leads of highway expansions:

(1c) $$\log L_{ijt} = \beta_1 H_{ijT} D_{<5km} + \beta_2 H_{ijT} D_{5-10km} + \alpha \log K_{ijt} + \gamma_i + \theta_t + \tau_j t_t + \varepsilon_{ijt},$$

where we emphasise that we use here H_{ijT} and where *T* takes the values: *t-5*, *t*, *t+1*, *t+3*. The effect of H_{ijt-5} might be larger or smaller than the effect captured by H_{ijt-3}. On the one hand, the long run effect may exceed the short run effect due to slow adjustment. On the other hand, an increase of congestion may lead to a decrease in the effect on local employment.

In case our model is correctly specified, then *contemporary* highway expansions, H_{ijt}, will have a lower effect on employment growth than H_{ijt-3} (because only a proportion of the highway expansions occur before the measurement of employment growth).[13] Furthermore, if our model is correctly specified, then future highway expansions should have no effect on employment growth. Hence, the inclusion of future expansions should be interpreted as a placebo effect.

4 Data

We focus on employment changes in the period 1996-2013, but use information about highway expansions before and after that period (from 1991-2016). We make use of three GIS-coded datasets: (i) A dataset of the Dutch highway system, 1991-2016 (made available by the Dutch Department of Transportation), which distinguishes between 260 highway

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[12] To test the idea that commercial real estate may endogenously react to highway expansions, we have analysed the effect of highway expansions on commercial real estate (see Appendix A). We show there that expansions within 5 kilometres have no significant effect on real estate buildings, which is in line with our finding that both specifications provide the same results.

[13] Given the assumption that the month of opening of the highway expansion is random throughout the year, the proportion is exactly half.

segments. For each segment we know its length (the average length is 8 km) and for each year the number of lanes (we do not know the exact month of the change in number of lanes). We calculate $H_{ijt}D_{<5km}$ and $H_{ijt}D_{5-10km}$for all 4044 (four digit) zip codes, i.e. the number of lane kilometres within 5 km as well as between 5 to 10 km from the centroid of a zip code.[14] (ii) A dataset 1996-2013 with zip code employment data (collected by LISA, a firm collecting employment data). It includes yearly information on full-time and part-time jobs and distinguishes between three types of sectors: production sector, commercial service and non-commercial services. (iii) A micro dataset containing all real estate properties in the Netherlands 1996-2013 (BAG), including information on floor space, function and location.

As explained above, we select zip codes that, during our study period, faced an expansion within a radius of 5 kilometres. To make sure that zip code areas are located close to ramps of highways, we only selected areas within a distance of 4 kilometres of a ramp (we also experimented with other distances). This results in a dataset including 1384 zip codes. We exclude zip codes which have experienced more than 3 expansions, which reduces the dataset to 20,340 observations annual for 1130 zip codes. Our main analysis will be based on areas with only one expansion during the study period which reduces the number of observations to 9360 annual observations for 520 zip Codes.

Table 1 reports the main descriptive statistics. At the end of the observation period, an average property in our data set had between 50 and 65 lane kilometres within a radius of 5 kilometres and another 110-130 lane kilometres within a radius of 10 kilometres. About 15% of these lane kilometres were added during our study period. The large standard deviation of these expansions we will exploit in our analysis.

Table 1 - Descriptive statistics

	Zip codes with one expansion only		*Zip codes with one to three expansions*	
	(i) mean	(ii) st.dev	(iii) mean	(iv) st.dev
Highway lane kilometre, 2013 (*radius 0-5km*)	52.59	22.21	63.83	30.86
Lane kilometre expansions, 1996-2013 (*radius 0-5km*)	6.91	7.82	10.52	11.01
Highway lane kilometre 2013 (*radius 5-10km*)	108.76	70.40	129.30	71.72
Lane kilometre expansions 1996-2013 (*radius 5-10km*)	15.92	16.74	19.98	16.95
Employment 2013 (*number of jobs x1000*)	2.13	2.70	2.55	3.19
Employment growth 1996-2013 (*%*)	0.37	0.60	0.34	0.57
Commercial real estate 2013 (*m^2x1000*)	109.21	174.16	119.04	192.56
Commercial real estate growth 1996-2013 (*%*)	0.40	0.61	0.38	0.54
Number of zip codes	520		1130	
Number of observations	9360		20340	

[14] These areas are small (a zip code covers an area of approximately 1 square kilometre).

5 Estimation results

5.1 Baseline estimates

Table 2 reports the baseline estimation results from regressions (1a) and (1b) for the sample of zip codes that faced one expansion during our study period. Models in the left panel include real estate supply in the zip code as an explanatory variable, models in the right panel do not. Standard errors are clustered at the zip code level.

Our results suggest that highway expansions positively influence the employment in the close vicinity. Adding 10 kilometre lane in the radius of 0 to 5 km of a zip code (this is 20% of the average highway supply in the close vicinity) leads within a time span of 3 years to some 3 % extra jobs in the zip code, on average. Highway expansions on a further distance (5 to 10 km) seem to have an opposite, negative, impact, but the results are smaller and not significant. The intuition for a negative coefficient is that a location loses its comparative advantage because competing nearby locations get highway expansions. The overall effect within a 10 kilometre radius is not significant, jobs thus relocate locally to places with improved accessibility. Models (1a) and (1b) yield very similar conclusions, suggesting that the commercial real estate variable is indeed uncorrelated with the highway variables.[15]

Table 2 – Baseline results

(dependent variable: the logarithm of employment per zip code)

	Model (1a)		Model (1b)	
	(i)	(ii)	(iii)	(iv)
	OLS	OLS	OLS	OLS
Lane length (*km x10*)				
radius 0-5km	0.030***		0.037***	
	(0.013)		(0.014)	
radius 5-10km	-0.012**		-.020***	
	(0.008)		(0.011)	
radius 0-10km		0.002		-0.000
		(0.004)		(0.005)
Supply commercial real estate (m^2, *log*)	0.425***	0.426***		
	(0.064)	(0.064)		
Zip code area fixed effects (520)	YES	YES	YES	YES
Year fixed effects (18)	YES	YES	YES	YES
Regional time trends	YES	YES	YES	YES
Observations	9360	9360	9360	9360
R-squared	0.48	0.48	0.39	0.39

Notes: We cluster standard errors at the neighbourhood level. Standard errors are in parentheses. *, **, ***, 10%, 5%, 1% significance, respectively.

[15] Appendix A shows that this is indeed the case.

We aim to compare our estimates with those from the literature on greenfield highways. Our results suggest that one standard deviation increase in the growth of lane length between 1996 and 2013 led to 3% local employment growth near the highways. Moeller and Zierer (2018) find for Germany that a one-standard-deviation increase in the growth of autobahn length between 1937 and 1994 led to employment growth of between 2.7 and 3.4% in the region where the highways were realised.

5.2 Lags and leads

Table 3 reports estimates for different lags and leads (equation (1c)). Again, the left panel includes real estate supply in the code as a regressor and the right panel does not. We include models with: contemporaneous highway accessibility, 5-year lag to account for slow adjustment, 1-year lead and 3-year lead to test for anticipation. If our model is correctly specified, the leads can be seen as a placebo.

Table 3 – Lags and leads
(dependent variable: the logarithm of employment per zip code)

	Model (1a)				Model (1b)			
	(i)		(ii)		(iii)		(iv)	
	OLS		OLS		OLS		OLS	
Lane length (*km x10*)								
radius 0-5km								
Lag 3	0.023**	(0.011)			0.025**	(0.010)		
Lag 5	0.013	(0.011)			0.020**	(0.010)		
Same year	0.005	(0.008)			0.008	(0.008)		
Lead1	0.003	(0.010)			0.001	(0.010)		
Lead 3	0.008	(0.010)			0.0008	(0.010)		
radius 5-10km								
Lag 3	-0.009**	(0.006)			-0.013**	(0.007)		
Lag 5	-0.006	(0.008)			-0.013	(0.010)		
Same year	-0.001	(0.005)			-0.005	(0.005)		
Lead1	-0.002	(0.005)			-0.002	(0.005)		
Lead 3	-0.008	(0.005)			-0.010	(0.007)		
radius 0-10km								
Lag 3			0.002	(0.004)			-0.001	(0.004)
Lag 5			0.001	(0.004)			-0.000	(0.005)
Same year			0.001	(0.004)			-0.000	(0.004)
Lead1			-0.001	(0.004)			-0.002	(0.004)
Lead 3			-0.003	(0.003)			-0.004	(0.004)
Commercial real estate (*m^2, log*)	0.424***	(0.064)	0.426***	(0.065)				
Zip code area fixed effects (520)	YES		YES		YES		YES	
Year fixed effects (18)	YES		YES		YES		YES	
Regional time trends	YES		YES		YES		YES	
Observations	9360		9360		9360		9360	
R squared	0.48		0.48		0.39		0.39	

Notes: We cluster standard errors at the neighbourhood level. Standard errors are in parentheses. *, **, ***, 10%, 5%, 1% significance, respectively.

The effect in the close vicinity appears weakly in the year of the opening and is at its strongest after 3 years. We find no significant anticipation effects.

5.3 Sensitivity analysis and robustness checks

We performed different sensitivity and robustness checks. The results are reported in Appendix B, table B1-B3.

Our data allow to distinguish different types of employment in the zip code: industry, commercial services and non-commercial service. Table B1 in Appendix B reports the results of estimating model (1a) for different types of jobs separately. The results do not differ significantly, although the point estimates suggest that industry jobs react most to the increased availability of a highway.

In table B2 we test the sensitivity of our results for the supply of the commercial real estate in a zip code at the beginning of the study period. The left panel reports the estimates for small locations (below the 20^{th} centile) and the right panel reports the estimates for large locations (above the 80^{th} centile). Again, the differences are not significant, but the point estimates suggest that for small locations, the effect of highway expansions may be stronger.

Finally, in Table B3 we enlarge our sample with zip codes that experienced two or three expansions during the study period. We estimate model (1c), accounting for various lags and leads. The size and the timing of the effect does not significantly differ from the baseline estimation, and the leads are again insignificant.

6 Conclusion

In this paper we have used unique longitudinal regional panel data on highway expansions 1991-2016 in the Netherlands to show that highway expansions lead to local job relocation. Employment in a zip code rises with 3% with every extra 10 kilometre lane length added nearby. This comes at the cost of employment further away, so that the total effect is zero. The effect appears within 3 years after the expansion. We have found no significant anticipation effects. The results are robust to various robustness checks.

Our results have important political economy implications. In developed countries, plans for construction and expansion of highways often meet fierce local protests. This local opposition has led to altering, postponing or even stopping proposed investments, to prevent expected negative externalities from pollution, noise and spatial barriers. We show that highway expansion can also result in considerable local benefits in terms of employment. These need to be accounted for when making a total balance of local costs and benefits of the highway investments.

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Appendix A

Table A1 Impact of highways on real estate supply

(dependent variable: the logarithm of m^2 real estate per zip code)

	Commercial real estate				Residential real estate			
	Coef.	. St.er.	Coef	St.er.	Coef.	St.er.	Coef.	St.er.
Lane km x10, lag3								
radius 0-5km	0.015	(0.014)			0.015	(0.011)		
radius 5-10km	-0.019*	(0.011)			-0.016	(0.011)		
radius 0-10km			-0.007	(0.005)			-0.006	(0.005)
R^2 within	0.47		0.47		0.36		0.36	
# obs.	9360		9360		8464		8464	
# zip codes	520		520		520		520	
pc4 fixed effect	YES		YES		YES		YES	
year fixed effect	YES		YES		YES		YES	
regional time trend	YES		YES		YES		YES	

Appendix B

Table B1 Impact of highways on employment by sector

(dependent variable: the logarithm of sectoral employment per zip code)

	Industry jobs		Commercial services		Non-comm.services	
Lane km x10, lag3						
radius 0-5km	0.060**		0.041***		0.037**	
	(0.029)		(0.015)		(0.021)	
radius 5-10km	-0.034		-0.012		-0.022	
	(0.025)		(0.010)		(0.015)	
radius 0-10km		-0.001		-0.007		-0.001
		(0.011)		(0.006)		(0.008)
m2 commercial real estate, log	0.390***	0.393***	0.462***	0.463***	0.340***	0.342***
	(0.085)	(0.085)	(0.073)	(0.074)	(0.079)	(0.079)
R^2 within	0.22	0.22	0.41	0.41	0.22	0.22
# obs.	9151	9151	9352	9352	9182	9182
# zip codes	520	520	520	520	520	520

Table B2 Impact of highways on employment, small and large locations

(dependent variable: the logarithm of employment per zip code)

	Small locations				Large locations			
	Coef.	St.er.	Coef.	St.er.	Coef.	St.er.	Coef.	St.er.
Lane km x10, lag3								
radius 0-5km	0.080*	(0.045)			0.023	(0.016)		
radius 5-10km	-0.008	(0.008)			-0.021	(0.016)		
radius 0-10km			0.025*	(0.014)			-0.002	(0.005)
m2 commercial real estate, log	0.266***	(0.067)	0.264***	(0.067)	0.268***	(0.068)	0.265***	(0.064)
R^2 within	0.66		0.66		0.64		0.64	
# obs.	1890		1890		1872		1872	
# zip codes	105		105		104		104	

Table B3 Impact of highways on employment,

(dependent variable: the logarithm of sectoral employment per zip code)

	Model 1a lagged highway, 3 year				Model 1b lagged highway, 3 year			
	Coef.	St.er.	Coef.	St.er.	Coef.	St.er.	Coef.	St.er.
Lane km x10 radius 0-5km								
Lag 3	0.018***	(0.006)			0.025***	(0.006)		
Lag 5	0.010*	(0.006)			0.022***	(0.007)		
Same year	0.007*	(0.004)			0.013***	(0.004)		
Lead1	-0.001	(0.003)			-0.001	(0.007)		
Lead 3	-0.004	(0.005)			-0.004	(0.005)		
radius 5-10km								
Lag 3	-0.010**	(0.004)			-0.016***	(0.005)		
Lag 5	-0.002	(0.005)			-0.011	(0.007)		
Same year	-0.003	(0.003)			-0.005	(0.004)		
Lead1	-0.003	(0.003)			-0.002	(0.003)		
Lead 3	0.001	(0.004)			0.001	(0.004)		
radius 0-10km								
Lag 3			0.001	(0.002)			0.003	(0.002)
Lag 5			0.003	(0.002)			0.001	(0.003)
Same year			0.002	(0.002)			0.002	(0.003)
Lead1			-0.002	(0.002)			-0.002	(0.002)
Lead 3			-0.001	(0.002)			-0.000	(0.002)
m2 commercial real estate, log	0.431***	(0.039)	0.436***	(0.039)				
R^2 within	0.44		0.44		0.34		0.33	
# obs.	20340		20340		20340		20340	
# zip codes	1130		1130		1130		1130	