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Spatial Externalities and Place-Based Policies

Evidence from the Netherlands

By HANS R.A. KOSTER* AND JOS VAN OMMEREN

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SUMMARY — We study neighbourhood externalities caused by large public investments in poor neighbourhoods. A stylised theory of a linear city is proposed to guide interpretation of the magnitude and attenuation of the external effects generated by these public investments. We use a large Dutch nationwide dataset and compare house price changes over time between targeted and control neighbourhoods. The control neighbourhoods are selected through a nonparametric propensity score method. The identifying assumption is that time-varying unobservable locational variables are uncorrelated to the choice of location for the revitalisation program. We relax this assumption by identifying the external effect based on the difference in price changes between targeted and control neighbourhoods *within* a municipality. To test for robustness of the results, we also identify the external effect *within* the neighbourhood. It is shown that public investments have led to an increase in house prices of about 3 percent. Using a novel method to estimate a decay parameter within the empirical model, it is shown that the external effect decreases with 50 percent every 250 metres from the targeted area.

JEL-code — R30, R33

Keywords — neighbourhood externalities; place-based policies; hedonic pricing;; spatial decay; housing market.

I. Introduction

We observe vast differences in the house price within cities due to heterogeneity in housing and location attributes. The price of a house is also determined by a variety of nonmarket interactions in its neighbourhood, to which we will refer as neighbourhood externalities. Differences in housing/location attributes and nonmarket interactions may generate income disparities and social problems related to segregation. It is therefore that in many countries *place-based policies* have been developed that make large investments in poor neighbourhoods.

Economists are often not in favour of these policies, as they argue that governments should help people, rather than places, and “not bribe people to live in poor places” (Glaeser, 2012). However, if nonmarket interactions are important, then this may justify place-based policies. For example, through local spillovers, a neighbourhood participation programme

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may decrease negative externalities (and increase house prices). There are few studies that confirm the presence and importance of these neighbourhood externalities (Ioannides, 2003; Schwartz et al., 2006; Rossi-Hansberg et al., 2010). This does not imply, however, that place-based policies are effective. For example, a number of studies, including Briggs et al. (1999), Lee et al. (1999) and Santiago et al. (2001), find no statistically significant, or even small negative effects, of place-based policies that subsidise housing. Also, investments in deprived inner cities in the US seem to be ineffective, as the income disparities in these cities have increased in the last decades (Mills and Lubuele, 1997).

In consequence, there is still limited understanding of the magnitude and nonlinearities in spatial externalities, and no consensus about the effect of place-based policies has been reached. It seems fair to say that any place-based policy is as likely to reduce as improve social welfare (Glaeser and Gottlieb, 2008). Hence, it is important to study a larger number of place-based policies in a large number of locations, to arrive at more robust conclusions.

It is the purpose of this paper to improve our understanding on the importance and spatial attenuation of neighbourhood external effects. It is well understood that such external effects cannot be distinguished from unobservable local attributes using solely the observed location decisions of households (Bayer and Timmins, 2007; Rossi-Hansberg et al., 2008). We therefore evaluate changes in house prices due to a large-scale nationwide urban revitalisation program in the Netherlands, starting in 2007. In this so-called ‘empowered neighbourhoods’ program (*krachtwijken*, henceforth KW-program), 83 postcode areas covering the most deprived urban neighbourhoods of the Netherlands, were selected to revitalise with funding from the national government.

The government and housing associations have invested more than a billion Euros in these areas since 2007 onwards, which is on average about two thousand Euros per household living in these neighbourhoods. The main objective of the program is to transform these neighbourhoods into pleasant places to live and reduce social inequality (Ministerie VROM, 2007). A large share of the investments is spend on restructuring of *public* housing stock. The remainder is used for investments in green spaces, social empowerment programs and the sale of public housing (Wittebrood and Permentier, 2011). The private housing stock, to which our data refer, was not restructured.

To guide the empirical analysis, we first develop a stylised theoretical model of a linear city with neighbourhood externalities. Households are assumed to maximise a utility function that depends on the consumption of house size, housing services (e.g., maintenance level of your house), and amenities, which capture housing services produced by neighbours (e.g., households prefer to live in a neighbourhood with better maintained houses). It is shown that due to an implicit subsidy on housing services, the consumption of housing services, the price of land, and therefore the amenity level increases, which is dependent on the level of housing services.

We utilise a nationwide dataset with (private) house transactions from 2003-2011 to test for the presence of neighbourhood externalities, using a novel estimation procedure to

estimate separate coefficients for the magnitude and decay of the external effect. We compare the price change over time in treated neighbourhoods with a group of control neighbourhoods. The control neighbourhoods are selected using a propensity score method, based on the selection criteria that were used to select the neighbourhoods for the revitalisation programme. Given the use of a propensity score method and a hedonic price specification with postcode six-digit (PC6) fixed effects (about a census block), the identifying assumption is that *time-varying* unobservable locational variables are uncorrelated to the choice of location for the revitalisation program. We relax this assumption by identifying the external effect based on the difference in price changes between targeted and control neighbourhoods *within* a municipality. To test for robustness of the results, we also identify the external effect *within* the neighbourhood.

We show that the externality effect of this investment program is statistically significant and economically meaningful. It is shown that house prices on average increase by about 3.5 percent due to the investment program. Importantly, we show that house prices outside the target areas also increase, implying external benefits of this program. The price increase is about 2.5 percent for houses that are at 250 meter of the target area. It is then shown that, due to the external effects, the benefits of the investments program exceed the investment costs of the program. We also test whether subsidies have a larger external effect in neighbourhoods with public housing, and this appears the case.

Our paper improves on the literature in several respects. First, we analyse a large nationwide national investment program, rather than focusing on one city or a specific neighbourhood in a city. Our identification strategy to identify external effects is seems strong: we use a nonparametric propensity score methods to select control neighbourhoods and control for a plethora of time-varying neighbourhood variables. Furthermore, we control for unobserved factors at the municipality level and even identify the external effect within postcodes. Third, rather than using arbitrary distance cut-offs, we estimate a spatial decay parameter within the empirical model.

The remainder of the paper is organised as follows. In Section II we describe the KW-policy, followed by theory that guides our empirical interpretation in Section III. Section IV discusses the data, the procedure to select the control neighbourhoods and the empirical methodology. Section V turns to the results, which is followed by the conclusions in Section VI.

II. The urban revitalisation policy

There is ample empirical evidence that households with low incomes and associated social problems are disproportionally located in certain urban neighbourhoods. For example, many US inner cities contain large concentrations of low-income households and score low on almost every measure capturing social dysfunction (Mills and Lubuele, 1997; Glaeser et al. 2008).

In the Netherlands, we observe a similar but less extreme pattern.¹ About 70 percent of the most deprived neighbourhoods are located in the four largest cities of the Netherlands (Amsterdam, Rotterdam, The Hague and Utrecht). The share of public housing is much higher in these neighbourhoods than in other parts of the Netherlands.² The gap between these poor neighbourhoods and other neighbourhoods in terms of unemployment, crime rates and income, has widened in the last decade. Therefore, in 2007, a substantial national investment program was launched by the Dutch secretary of state, who was responsible for living, working and integration. About € 250 million was invested in the 83 worst performing neighbourhoods in the Netherlands, about € 750 per household living in the neighbourhoods. The money was used to assist municipalities in restructuring and revitalisation of neighbourhoods. In September 2007, the secretary of state agreed with large public housing associations that they would invest another € 750 million in the selected neighbourhoods. Although we do not know the exact total amount of money that was invested, at least one billion euros has been invested in these neighbourhoods between 2007 and 2011. Apart from physical restructuring of public rental housing and sale of public housing, the investments were also targeted at poor households directly through empowerment programs (Ministerie VROM, 2007; Wittebrood and Permentier, 2011).

The list of targeted neighbourhoods was not made public because the secretary of state was afraid of a negative stigmatisation effect of these neighbourhoods (which may have reduced house prices). So, households could not have anticipated the investments. Nevertheless, in late 2008 under pressure of the press, the secretary of state was forced to disclose the list of neighbourhoods with the exact ranking and selection criteria. The selection criteria encompass neighbourhood income, unemployment level, social integration, quality of the housing stock, crime indicators and residential satisfaction. It is important to note that the house price was *not* a selection criterion. There was substantial criticism on the selection of the specific neighbourhoods. According to opponents, the selection criterions were randomly chosen and the postcode areas were too large to capture meaningful neighbourhoods. Nevertheless, only one neighbourhood on this list was replaced by another neighbourhood originally not on the list.

III. Neighbourhood externalities: theory

A. A model with neighbourhood externalities

In this section, we provide a theoretical framework that helps to interpret the empirical outcomes. We develop a stylised model of a linear neighbourhood. It has the following features. First, it allows for households to trade-off house size, housing services and neighbourhood externalities, which depend on the housing services produces by others in

¹ Due to substantial labour market programs and progressive taxation, differences in household income are less pronounced than in the US.

² Public housing is common in the Netherlands, about 35 percent of Dutch residences are public housing, which is by far the highest in Europe.

vicinity. Second, households are shown to consume more housing services when a housing services subsidy is implemented, leading to a higher amenity level throughout the city.

Identical households occupy one location ℓ in a city $\mathcal{C} = [-R, R]$ where R is the edge of a city. A household transforms one unit of labour in w units of the final good. Households maximise a constant returns to scale Cobb-Douglas utility function, consisting of housing services $h(\ell)$, house size $s(\ell)$ and a public good, which we refer to as an amenity, $a(\ell)$. Housing services $h(\ell)$ are private investments made in the house (or neighbourhood) and may refer to maintaining the house, or participating in social neighbourhood activities. We then have:

$$(1) \quad \max_{h(\ell), s(\ell)} u(\ell) = h(\ell)^\alpha s(\ell)^\beta a(\ell)^\gamma, \quad \alpha + \beta + \gamma = 1, \quad \alpha, \beta, \gamma < 1$$

subject to a budget constraint:

$$(2) \quad w = p(\ell)s(\ell) + h(\ell)$$

where $p(\ell)$ is the price per unit of house size (and the price of housing services is normalised to one). We emphasise that amenities $a(\ell)$ are given for the households, but are dependent on housing services consumed in nearby locations. Following Lucas and Rossi-Hansberg (2002) and Rossi-Hansberg et al. (2010), we define:

$$(3) \quad a(\ell) = \delta \int_{-R}^R e^{-\delta|\ell-s|} h(s) ds,$$

where δ is a decay parameter. It may be shown that households will choose $h(\ell)^*$ such that:³

$$(4) \quad h(\ell)^* = h^* = \frac{\alpha w}{\alpha + \beta}.$$

We then substitute $h(\ell)^*$ into (3) to obtain:

$$(5) \quad a(\ell)^* = \frac{\alpha w \delta}{\alpha + \beta} \int_{-R}^R e^{-\delta|\ell-s|} ds,$$

where $a(\ell)^*$ refers to the equilibrium amenity level, given that households maximise utility. Note that $a(\ell)^*$ depends on ℓ . For example, the above equation implies that the amenity value is always higher at central locations, because $\partial a(\ell)/\partial |\ell| < 0$.

We assume free residential mobility of households within the city. This implies that utility is equalised across the city, so $u(\ell) = \bar{u}$, which is the reservation utility. Based on the optimality conditions associated with (1), it may be shown that the price per unit of housing is:

$$(6) \quad p(\ell)^* = \left(\delta \int_{-R}^R e^{-\delta|\ell-s|} ds \right)^{\frac{\gamma}{\beta}} \beta \alpha^{\frac{1-\beta}{\beta}} \left(\frac{w}{\bar{u}\gamma} \right)^{\frac{1}{\gamma}},$$

So, prices are higher at central locations. To obtain the chosen house size at a particular location, we use (5) and obtain:

³ The asterisks indicate the equilibrium outcome.

$$(7) \quad s(\ell)^* = w^{\frac{\gamma-1}{\gamma}} \alpha^{\frac{\beta-1}{\beta}} (\bar{u}\gamma)^{\frac{1}{\gamma}} \left(\delta \int_{-R}^R e^{-\delta|\ell-s|} ds \right)^{-\frac{\gamma}{\beta}}.$$

So, households substitute amenities for house size, implying that houses are generally smaller in neighbourhoods with high amenity levels.

Equations (4), (5), (6) and (7) define the equilibrium, which is a function of housing investments $h(\ell)^*$, house size $s(\ell)^*$, the amenity level at all locations $a(\ell)^*$ and the price per unit of housing $p(\ell)^*$ at all locations.⁴ Lucas (2001) shows the existence and uniqueness of an equilibrium for a similar class of models.

Now consider a certain investment program π to subsidise housing services in the inner city. The subsidy is ad-valorem and is equal to $\sigma(\ell)h(\ell)$, which implies that households effectively pay $1 - \sigma(\ell)$ for one unit of housing services. We refer to the target area as $\mathcal{T} = [-r, r]$. The budget constraint then reads $w = p(\ell)s(\ell) + (1 - \sigma(\ell))h(\ell)$, where $\sigma(\ell) > 0$ if $\ell \in \mathcal{T}$ and $\sigma(\ell) = 0$ otherwise. The amenity value at a certain location is then given by:

$$(8) \quad a_{\pi}(\ell)^* = (1 - \beta)w\delta \int_{-R}^R \frac{e^{-\delta|\ell-s|}}{1 - \sigma(s)} ds.$$

So, the price per unit of housing at a certain location becomes:

$$(9) \quad p_{\pi}(\ell) = (1 - \sigma(\ell))^{-\frac{\alpha}{\beta}} \left(\delta \int_{-R}^R \frac{e^{-\delta|\ell-s|}}{1 - \sigma(s)} ds \right)^{\frac{\gamma}{\beta}} \beta \alpha^{\frac{1-\beta}{\beta}} \left(\frac{w}{\bar{u}\gamma} \right)^{\frac{1}{\gamma}}.$$

The log price difference at a location due to the investment program is then given by:

$$(10) \quad \log \left(\frac{p_{\pi}(\ell)}{p(\ell)} \right) = \frac{\alpha}{\beta} \log(1 - \sigma(\ell)) + \left(\frac{\gamma}{\beta} \right) \log \frac{\int_{-R}^R \frac{e^{-\delta|\ell-s|}}{1 - \sigma(s)} ds}{\int_{-R}^R e^{-\delta|\ell-s|} ds}.$$

The first term on the right-hand side is the direct effect of the investment program. Because $\sigma(\ell)$ is zero for $\ell \notin \mathcal{T}$, the direct effect due to the investment program is zero in non-targeted areas. In our empirical analysis, we will ignore the direct effect because these are likely to be negligible in the public investment program analysed in the current paper (residences that are directly influenced by the investment are public and are not included in our sales dataset). Furthermore, the increase in production of own housing services must be negligible compared to the increase in the total production of housing services in a city, when the city is sufficiently large.⁵ The second term is the effect due to the externality. We focus on identifying the latter effect in the empirical analysis.

⁴ It is possible to define the boundaries of the city endogenously by defining the price of e.g. agricultural land $p(-R) = p(R)$. It may then be shown that the city will grow when subsidies have taken place because the land rents at the boundary increase.

⁵ In addition, we will compare house price changes just outside the targeted neighbourhoods for which the direct effect is guaranteed to be zero.

B. Stigmatisation effects

[EXTENSION OF THE MODEL ... TO BE DISCUSSED]

IV. Empirical framework and data description

A. Data and control neighbourhoods

Our analysis is based upon a house transactions dataset from the NVM (Dutch Association of Real Estate Agents). It contains information on about 80 percent of all transactions between 2003 and 2011.⁶ For 1,394,856 transactions, we know the transaction price, the exact location, and a wide range of house attributes such as size (in square meters), type of house, number of rooms and construction year.⁷

For each postcode 6-digit (PC6) area we estimate $a_{\ell t}(\delta)$.⁸ We also gather neighbourhood data on the share of owner-occupied houses, the average income, population density, the share of ethnic minorities and the share of young people (less than 25 years) and elderly (more than 65 years).⁹ We do not have detailed information on the share of public housing but in the Netherlands, only 10 percent of rental houses are privately owned, implying that one minus the share of owner-occupied housing captures the share of public housing reasonably well. Furthermore, in poor neighbourhoods private rental houses are almost always rent-controlled. Consequently, the private rents will not change due to a public investment program.

To correct for arbitrary neighbourhood boundaries, we calculate spatially-weighted variables, similar to the approach of Banzhaf and Walsh (2010). For example, population density is measured as $v_{\ell t} = \sum_{s=1}^S e^{-\theta d(\ell, s)} h_{st}$, where h_{st} is the number of people at a certain location s and θ is a decay parameter, and $d(\cdot)$ is measured in kilometers. We will assume that $\theta = 10$, so we focus on the local population density (within 100 metres from the own location). The shares are calculated by calculating the number of foreigners, young and old people in the vicinity using the same exponential weighting function.

Obviously, the targeted neighbourhoods are not randomly assigned, because the neighbourhoods are selected based on certain criteria. We will therefore use a propensity score method to select similar control neighbourhoods. Rosenbaum and Rubin (1983) propose to estimate a flexible probit model, where a dummy indicating whether a neighbourhood is selected is regressed on a flexible function of covariates, including relevant selection criteria. Based on the idea that neighbourhoods that have similar propensity scores

⁶ In the (large) cities we focus on, the NVM has a more dominant position, so the 80 percent is likely an underestimate. The figure may be as high as 90 percent.

⁷ We exclude transactions with prices that are above € 1.5 million or below € 25,000 or a square meter price below € 250 or above € 5,000. Furthermore, we exclude transactions that refer to properties smaller than 25m² or larger than 300m².

⁸ A PC6 area contains on average 20 households and is comparable to a census block in the United States.

⁹ Neighbourhoods are fairly small: the average distance to the centroid of a neighbourhood is only 286 meter.

are similar in its characteristics, the propensity score is used to match targeted and control neighbourhoods.

To apply a propensity score methods, we gather data from Statistics Netherlands on population density, average income, share of people with low income, the share of unemployed people, and the share of households that receive social allowance in 2007 at the neighbourhood level. To capture the degree of social integration, we furthermore include the share of foreigners, the share of young people (<25 years) and share of elderly (>65) years. The quality of the housing stock is measured by the median construction year, as well as the share of houses that are constructed before 1945 and between 1945 and 1970 (houses in the latter category are thought to have lower quality). We also include a variable indicating the share of open space in the neighbourhood, as well the share of owner-occupied houses. We then estimate the following probit model:

$$(11) \quad \Pr(n = 1 \mid z_n) = \Phi(Y_n(z_n)),$$

where $\Pr(n = 1 \mid z_n)$ is the probability that a neighbourhood n is selected, $\Phi(\cdot)$ is the cumulative distribution function of the normal distribution and $Y_n(\cdot)$ is a nonparametric function of attributes z_n . $Y_n(\cdot)$ is estimated by using local likelihood estimation, implying that we estimate for each neighbourhood a weighted probit model (see Fan et al., 1995; 1998). We let the weights depend on geographical location to capture unobserved spatial heterogeneity.¹⁰

To select the control neighbourhoods, we use nearest neighbour matching with replacement (Rosenbaum and Rubin, 1985; Rosenbaum, 2002). This implies that we minimise the absolute differences in propensity scores between the targeted and control neighbourhoods. Because we allow for replacement, it also implies that the number of control neighbourhoods might be lower than the number of targeted neighbourhoods, because multiple targeted neighbourhoods may be matched to one control neighbourhood.

Table 1 presents the means $\hat{\mu}$ and standard deviations $\hat{\sigma}$ at the neighbourhood level. It appears that the 38 control areas are relatively similar to the 83 target areas in most neighbour attributes. There are two notable differences between the targeted and control neighbourhoods. The first is that population density is about a third lower in the control neighbourhoods. Indeed, targeted areas are on average located in larger cities. Also, the share of foreigners is about 11.4 percentage points lower. The propensity scores are very close to zero in the other areas (0.004), suggesting that our model performs reasonably well.

A major assumption of the propensity score matching method is that unobservables are uncorrelated with the treatment, which may not hold in the current application. For example, we do not observe crime rates. When crime rates grow faster in targeted neighbourhoods (compared to control neighbourhoods), house prices in targeted areas may decline relative

¹⁰ So, the impact of z_n on $\Pr(n = 1 \mid z_n)$ depends on the location of the neighbourhood. The kernel weights for n are equal to $\omega_n = 1/d_n$, where d_n is a vector capturing the kilometre distance between the centroid of n and the centroids of all other locations (see similarly Fotheringham et al., 2002)

to house prices in the control neighbourhoods. We relax the assumption of zero correlation between time-varying unobserved heterogeneity and the effect of interest in three ways. First, we will include control variables at the neighbourhood level, such as population density, neighbourhood income, the share of owner-occupied housing and the share of foreigners in the hedonic specification. Second, we will estimate a specification that compare price changes of targeted and control neighbourhoods *within* the municipality. Third, we also identify the external effect *within* the neighbourhoods, so that we effectively control for all spatial heterogeneity between neighbourhoods.¹¹ The disadvantage of the last two approaches is that they are less efficient, implying that the estimates of the decay parameter δ become imprecise.

TABLE 1 — PROPENSITY SCORE MATCHING

	Targeted areas		Control areas		Other areas	
	$\hat{\mu}$	$\hat{\sigma}$	$\hat{\mu}$	$\hat{\sigma}$	$\hat{\mu}$	$\hat{\sigma}$
Population density (ha^2)	9081	5171	6804	4476	1411	2422
Income	10965	1049	11669	1263	13263	1999
Median construction year	1950	24	1953	22	1967	41
Share owner-occupied housing	0.225	0.092	0.244	0.088	0.679	0.150
Share foreigner	0.459	0.180	0.345	0.194	0.050	0.072
Share young	0.333	0.044	0.304	0.048	0.299	0.062
Share elderly	0.123	0.050	0.158	0.080	0.144	0.063
Share open space	0.170	0.158	0.191	0.171	0.689	0.316
Share social allowance	0.224	0.038	0.215	0.047	0.122	0.042
Share unemployed	0.367	0.059	0.342	0.056	0.196	0.063
Share low income	0.471	0.047	0.452	0.047	0.402	0.055
Share houses constructed <1945	0.326	0.317	0.304	0.256	0.214	0.187
Share houses constructed 1945-1970	0.354	0.304	0.377	0.303	0.250	0.164
Propensity score	0.622	0.337	0.399	0.281	0.004	0.040
Number of neighbourhoods	83		38		3,890	
Number of housing transactions	44,851		21,599		1,070,517	

Note: The analysis is done at the neighbourhood level. The number of observations is 4,011. To select the control neighbourhoods, we use nearest neighbour matching without replacement.

B. Empirical estimation procedure

We denote the location of a house using coordinates, so $\ell = (x, y) \in R^2$. We then use a standard hedonic price approach where the log price per square meter of a house at location ℓ year t , $p_{\ell t}$, is a function of the amenity value at a certain location $a_{\ell t}$, housing attributes $x_{\ell t}$, neighbourhood attributes $z_{\ell t}$, and a year fixed effect v_t . To control for all unobserved time-invariant neighbourhood attributes, we include a postcode six-digit fixed effect ξ_ℓ . So:

$$(13) \quad p_{\ell t} = \rho a_{\ell t}(\delta) + \lambda x_{\ell t} + \mu z_{\ell t} + \xi_\ell + v_t + \epsilon_{\ell t},$$

where ρ measures the impact of the amenity (and therefore of the policy), λ and μ are the coefficients of the control variables to be estimated. $\epsilon_{\ell t}$ is an identically and independently distributed error term.

¹¹ This implies that the choice of control neighbourhoods through the propensity score method is of limited importance.

Following the theoretical analysis, we assume that amenities are captured by a spatially weighted average of targeted locations in the neighbourhood, where $s = 1, \dots, S$:

$$(14) \quad a_{\ell t}(\delta) = \delta \sum_{s=1}^S e^{-\delta d(\ell, s)} q_{st},$$

where q_{st} denote a dummy variable that equals one when location s in year t is part of a targeted area, $d(\ell, s)$ denotes the kilometre distance between ℓ and s , and δ denotes the decay parameter so $a_{\ell t}$ varies over space. For this reason, we normalise $a_{\ell t}$ in such a way that the average $a_{\ell t}$ in target areas is equal to one. We assume that the investment is homogeneous (within the targeted neighbourhoods). However, this implies that locations that are more centrally located in a targeted neighbourhood have higher $a_{\ell t}(\delta)$.¹² We estimate δ simultaneously with ρ , λ , μ , ξ_{ℓ} and v_t by minimising a cross-validation score (see also Koster, 2013). Minimising a cross-validation score is often used in the determination of the smoothing parameter in nonparametric and semiparametric estimation (Cleveland, 1979; Bowman, 1984; Farber and Páez, 2007; McMillen and Redfearn, 2010). In regression-discontinuity designs, a cross-validation procedure is frequently employed to determine the bandwidth (Imbens and Lemieux, 2008; Lee and Lemieux, 2010).¹³ We define the cross-validation criterion as:

$$(15) \quad \mathcal{CV}(\delta) = \sqrt{\frac{1}{S} \sum_{j=1}^S (p_{\ell t} - \hat{p}_{\ell t}(\delta))^2}.$$

So, $\mathcal{CV}(\delta)$ is the root mean-squared error for a given decay parameter. The corresponding decay parameter choice is then given by:

$$(16) \quad \{\delta^*\} = \arg \min \mathcal{CV}(\delta).$$

The standard error of ρ and δ are determined by bootstrapping this estimation procedure.

We propose two generalisations of equation (13). The first generalisation relaxes the key identifying assumption. It may be argued that control neighbourhoods that are not in the same city are potentially not comparable to each other. This increases the possibility of the presence of time-varying unobservables that are correlated with $p_{\ell mt}$ and $a_{\ell mt}(\delta)$. We therefore also include municipality×year fixed effects, which controls for time-varying unobservables at the municipality level:

$$(17) \quad p_{\ell mt} = \rho a_{\ell mt}(\delta) + \kappa x_{\ell mt} + \lambda z_{\ell mt} + \xi_{\ell m} + v_{mt} + \epsilon_{\ell mt},$$

where the subscript m denotes the municipality and v_{mt} denote the municipality×year fixed effects.

¹² If investments are clustered within locations in the targeted neighbourhood (e.g. one street receives all investments), then we measure $a_{\ell t}(\delta)$ with random measurement error, implying an underestimate for ρ .

¹³ One may also use other criteria to determine optimal the bandwidth δ . For example, the Akaike Information Criterion (AIC) and the Bayesian Information Criterion (BIC) are often used (see e.g. Hurvich et al., 1998). It may be shown that these criteria will lead to exactly the same results.

One may argue that this procedure does not distinguish between the direct effects of the investments and the externality effect (as emphasised in equation (10)). To separate the direct effect from the externality we also estimate an equation where the observations that are part of the target area are excluded, so we only include control neighbourhoods.

Furthermore, unobserved spatial variables that have influenced the treatment of neighbourhood may bias the results. We therefore also estimate a specification with neighbourhood \times year fixed effects v_{nt} , implying that we identify the external effect *within* neighbourhoods. This estimation procedure identifies ρ , because $a_{\ell t}(\delta)$ varies within neighbourhoods: locations in the centre of \mathcal{T} have a higher value of $a_{\ell t}(\delta)$.

The second generalisation refers to the heterogeneity in the effect of the investments ρ . It can be seen that the previous equations do not allow for heterogeneity in the effect of investments. Specifically, it may be expected that the effect is more important in areas with a higher share of public housing as a large share of the budget was spend on the improvement of public housing. We estimate:

$$(18) \quad p_{\ell mt} = \rho_{\ell} a_{\ell mt}(\delta) + \kappa x_{\ell mt} + \lambda z_{\ell mt} + \xi_{\ell m} + v_{mt} + \epsilon_{\ell mt},$$

where

$$\rho_{\ell} = \rho^0 + \sum_{v=1}^V \rho^v (z_{\ell mt}^v - \bar{z}_{\ell mt}^v).$$

Thus, the effect of the investment is a function of demeaned neighbourhood attributes $(z_{\ell mt}^v - \bar{z}_{\ell mt}^v)$, where $v = 1, \dots, V$, and V denotes the number of demeaned neighbourhood attributes. (similar to Bayer et al., 2007).

V. Results

A. Regression results

We regress house price on neighbourhood externalities, proxied by $a_{\ell t}$. We start with some standard linear regressions using the full dataset. Then, we will focus on the targeted neighbourhood and control neighbourhoods. The results are presented in Table 1. We estimate the standard errors using a bootstrapping procedure (100 replications). To account for clustering at the neighbourhood level, the sample drawn during each replication is a bootstrap sample of neighbourhoods.

In the first two specifications, where we use the full dataset and postcode fixed effects, we do not aim to identify a causal effect. Nevertheless, these specifications are useful to provide information about whether there is a strong correlation of the external effect with time-varying observable variables. In Specification (1) we include only postcode and year fixed effects. We find that that the investment program increases house price on average with 6.9 percent in targeted areas. The decay parameter is small and statistically insignificant, suggesting that there is hardly any decay in the externality effect, which is implausible. In Specification (2), we also include housing and time-varying neighbourhood attributes. The results are then very similar, although the externality effect is slightly lower now. We

emphasise that by including the control variables, the estimates become much more precise. This suggests that many control variables are useful, not so much for consistency, but mainly for efficiency reasons.

TABLE 1 — REGRESSION RESULTS: MEASURING NEIGHBOURHOOD EXTERNALITIES
(Dependent variable: the logarithm of the house price per square meter)

		(1)	(2)	(3)	(4)	(5)	(6)
Externality effect, $a_{\ell mt}$	$\hat{\rho}$	0.069 (0.033) **	0.058 (0.006) ***	0.045 (0.008) ***	0.033 (0.007) ***	0.064 (0.016) ***	0.038 (0.005) ***
	$\hat{\delta}$	0.300 (0.466)	0.300 (0.047) ***	2.800 (1.060) ***	3.400 (1.500) **	2.300 (1.014) **	2.600 (1.862)
		No	Yes	Yes	Yes	Yes	Yes
Housing attributes, $x_{\ell mt}$ (16)		No	Yes	Yes	Yes	Yes	Yes
Neighbourhood attributes, $z_{\ell mt}$ (6)		No	Yes	Yes	Yes	Yes	Yes
PC6 fixed effects, ξ_{ℓ}		Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects, v_t		Yes	Yes	Yes	Yes	Yes	Yes
Municipality×year fixed effects, v_{mt}		No	No	No	Yes	Yes	Yes
Neighbourhood×year fixed effects, v_{nt}		No	No	No	No	No	Yes
Cross-validation score $\mathcal{CV}(\delta^*)$		0.1675	0.1395	0.1157	0.1067	0.1107	0.1053
Adjusted- R^2		0.7165	0.8032	0.8879	0.8757	0.8570	0.8781
Number of observations		1,136,967	1,136,967	66,450	66,450	21,869	66,450

Notes: Standard errors are clustered at the neighbourhood level and bootstrapped (100 replications).

*** Significant at the 0.01 level

** Significant at the 0.05 level

* Significant at the 0.10 level

In Specification (3) we only include observations that are either in the targeted or control neighbourhoods. This estimation strategy is more sensible, because it implies that we compare the price change in a targeted neighbourhood with price changes in control neighbourhoods. The estimate of the external effects of neighbourhood investments drops somewhat (compared to Specification (2)), but is still important: targeted neighbourhoods have experienced on average a price increase of 4.5 percent. The estimate of the decay parameter δ is now more realistic and suggests that the externality effect is locally important: the decay function implies that at a 250 meter distance from the targeted area, the price increase is 2.4 percent, so about half.¹⁴

In Specification (4) we relax the identifying assumption that time-varying unobservables are correlated with the treatment by including municipality×year fixed effects, so we control for all time-varying unobservable variables that change at the municipality level. This implies that we compare the price change in a targeted neighbourhood with a price change in a control neighbourhood *within* the same municipality. The effect of the investment programme is now slightly lower. A house has become 3.3 percent more expensive in the targeted neighbourhood. After 250 meters, this effect is 1.4 percent (about 45 percent).

One may argue that we may capture the direct effect of the subsidies, as the subsidy may have caused a change in the housing services produced in the selected neighbourhoods (see

¹⁴ Note that the standard errors hardly increase compared to (2), implying that using ‘only’ 38 control neighbourhoods is more than sufficient.

equation (9)). We therefore excluded the targeted areas in Specification (5). It is shown that the externality effect is *higher* than the coefficient in Specification (4), suggesting that the direct effect is negative or that there is a stigmatisation effect of the programme (which was one of the main reasons that the secretary of state did *not* want to make the list of targeted neighbourhoods public).¹⁵ So, our previous estimate of the external effect is likely an underestimate. Importantly, the decay parameter is similar to Specification (4). This is important because it suggests that our assumption that investments are homogeneous within targeted areas seems adequate.

A concern is still that the set of control neighbourhoods are not appropriate in controlling for time-varying unobservable variables. We therefore include neighbourhood×year fixed effects, implying that we compare price changes *within* a postcode (PC4) area. Specification (6) shows that the external effect is very similar and 3.8 percent. The decay parameter has a similar value to the previous specifications, but because of the large number of spatial fixed effects difficult to identify and less precisely estimated.

So, these results point towards an economic meaningful external effect of the investment programme. The magnitude is comparable to what is found in previous studies (see Rossi-Hansberg et al., 2010), although the local conditions are quite different, as well as the geographic scope of the investment programme (we focus on average effects for 83 neighbourhoods, rather than three neighbourhoods in Rossi-Hansberg et al., 2010).

B. Robustness

[TO BE EXTENDED...]

TABLE 2 — REGRESSION RESULTS: ACCOUNTING FOR HETEROGENEITY
(Dependent variable: the logarithm of the house price per square meter)

		(1)	(2)	(3)	(4)
Externality effect, $a_{\ell mt}$	$\hat{\rho}$	0.038 (0.006) ***	0.030 (0.006) ***	0.048 (0.013) ***	0.036 (0.009) ***
	$\hat{\delta}$	2.900 (1.223) **	3.600 (1.223) ***	2.600 (1.713)	2.800 (1.038) ***
				0.023 (0.010) **	0.018 (0.007) **
Externality effect × population density (<i>log</i>)				-0.020 (0.058)	-0.092 (0.051) *
Externality effect × income (<i>log</i>)				-0.250 (0.056) ***	-0.162 (0.042) ***
Externality effect × share own	-0.267 (0.044) ***	-0.164 (0.038) ***		-0.014 (0.051)	-0.075 (0.051)
Externality effect × share foreigner				-0.276 (0.194)	-0.306 (0.174) *
Externality effect × share young				-0.244 (0.120) **	-0.361 (0.067) ***
Externality effect × share old					

¹⁵ When we re-estimate Specification (4) including a dummy whether a house is part of a targeted area, the dummy is statistically significantly negative (-0.035), which is in line with Specification (5).

Housing attributes, $x_{\ell mt}$ (16)	Yes	Yes	Yes	Yes
Neighbourhood attributes, $z_{\ell mt}$ (6)	Yes	Yes	Yes	Yes
PC6 fixed effects, ξ_ℓ	Yes	Yes	Yes	Yes
Year fixed effects, v_t	Yes	Yes	Yes	Yes
Municipality×year fixed effects, v_{mt}	No	Yes	No	Yes
Cross-validation score $\mathcal{CV}(\delta^*)$	0.1152	0.1066	0.1150	0.1063
Adjusted- R^2	0.8733	0.8760	0.8738	0.8766
Number of observations	66,450	66,450	66,450	66,450

Notes: See Table 1.

C. What are the total benefits of the investment program?

We do not know exactly how much has been invested in each neighbourhood, but by estimating the total benefits we may have an idea whether the policies have been beneficial. We use the baseline results of Specification (4), so $\hat{\rho} = 0.033$ and $\hat{\delta} = 3.400$. We then use a dataset with information on all residential buildings in the Netherlands, obtained from the Administration of Addresses and Buildings (*Basisadministratie Adressen en Gebouwen*). We have information on the total size of each residence in the Netherlands.¹⁶ Using this building dataset, we compute the total square meters of living space per PC6 area. We then compute the average price for each postcode location using an exponential function, so $\hat{p}_\ell = \sum_{s=1}^S e^{-\theta d(\ell,s)} p_s / \sum_{s=1}^S e^{-\theta d(\ell,s)}$, where $\theta = 10$. Using the value of the amenity increase for each postcode and given $\hat{\rho}$ and $\hat{\delta}$ we can now compute the total price increase.

The total price increase is equal to about XX billion euro (in 2007 prices). So, in line with Rossi-Hansberg et al. (2010), the investments are fully offset by the investment program. In other words, when neighbourhood externalities are important, investing in poor neighbourhoods seems a lucrative investment from a social welfare point of view.

V. Conclusions

In many countries, governments invest in poor neighbourhoods to narrow income disparities within cities. Because there is limited understanding whether neighbourhood externalities are important it is unknown whether these policies are welfare improving. In the current paper, we investigate the magnitude and attenuation of neighbourhood externalities using a nationwide investment program that aims to restructure and revitalise the poorest neighbourhoods in the Netherlands.

Using data on house sales in the period 2003-2011 we find compelling evidence for the presence of neighbourhood externalities. It is shown that the investment programme leads to an increase in house prices of about 3 percent, which is line with previous findings by Rossi-Hansberg et al. (2010). We calculate the total benefits and it seems that the investment has increased welfare substantially, although we do not know the exact total amount of money invested in the targeted neighbourhoods.

¹⁶ We delete all residences with a size lower than 25m² and larger than 250m².

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Appendix A. Exponential decay function

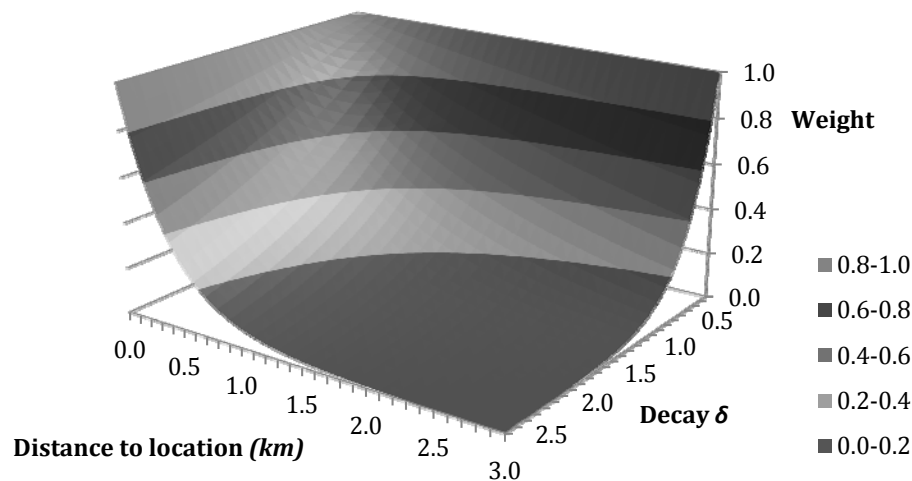


FIGURE A1 — THE EXPONENTIAL DECAY FUNCTION